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ORIGINAL COMMUNICATIONS.

Contributions to Obstetrics, with Tabular Views, and Miscellaneous Practical Observations. By Henry A. Ramsay, M. D., Raysville, Georgia.

Contributions to obstetrical science from the South are so rare, that I have concluded to present the results of my restricted experience, in this department, in a tabular and commentative form, which, however lame in practical importance, may elicit farther details, and furnish some data for determining the comparative state of rational midwifery in the Southern country. The field of obstetrical observation in the South being confined largely to our black population, presents at many points advantages for the practical elucidation of obstetricy nowhere else to be obtained by the professional man. At the present day a large majority of the obstetrical cases among this class, are exclusively under the control of the physician, and I am happy to add, the white ladies of Georgia are fast falling into ranks, in reference to this important point. Wherever our black population is dense, and their condition healthful, they increase with an almost unparalleled rapidity, and are unusually prolific; and such is the care with which they are provided for by their owners, to secure these desirable ends,

that we hazard nothing in asserting that the negroes of Georgia are better provided for, *obstetrically* and *dietetically*, than any other dependent class upon this continent, *private infirmaries*, *public hospitals*, *alms-houses*, *pauper cliniques*, *and lying in hospitals*, to the contrary, notwithstanding. The planters have their family physicians, who have the exclusive control of the blacks, and who are called without reference to simple or emergent cases, and without regard to expense. Since my advent into the profession, it has been my good fortune to attend several hundred cases of labor, embracing almost every variety, from the simplest to the most difficult forms; and it has been my better luck not to have sustained a *single loss*. In explanation of this success, it may be remarked that our blacks are well fed, clothed, and favored as to labor, and our white ladies enjoy in an eminent degree, all those luxuries and advantages which conspire to health; they are therefore stout, well formed, and energetic, having seldom any of those pelvic deformities or other causes, which induce laborious or tedious labor; consequently they conceive readily, and bring forth with marked facility. It is true, that we sometimes meet with difficult and fatal labors among both colors, but they are usually dependent upon *too early marriages*, *accidents*, or other causes beyond the ken of human comprehension or judgment. Such cases will occur among all classes and colors, and in any community. It will be seen by reference to these tables, that puerperal convulsions are quite rare, at least in this section of Georgia, and I apprehend that what is common here is true of other parts of the State. An interesting enquiry presents itself in reference to this point, should it be farther corroborated:—What is the cause of this immunity? Hypothetical disquisition is not the object of this paper, but I would make a suggestive rationale of the causation of this exemption, without fully endorsing or denying its validity—*that it is dependent upon a preponderance of the cerebral over the spinal system.* This is a fruitful and speculative theme, involving many points of acquiescence and contrariety, but I am inclined to think many sound views may be urged in justification of the position, whether the postulate be correct or not. But I will reserve these views for another occasion, and descend to a tabular statement of my cases, appending such remarks as I may deem necessary.

EUTOCIA.

Natural, { 429 vertex presentations,
5 twin cases,

Unnatural, { 3 footlings,
4 breech,
1 facial.

Complicated, {
3 cases arm and shoulder presentation,
1 case arm, foot and placenta,
2 cases placenta previa,
1 case side and funis presenting,
4 cases puerperal convulsions,
1 case tumor impeding labor,
1 case labial effusion,
2 cases hæmorrhage prior to labor,
1 case adherent placenta,
3 cases hour glass contraction,
1 case cord torn from placenta in utero,
1 case impacted head.

DYSTOCIA.

Manual,—3 cases required turning.

Instrumental, { 2 cases required forceps,
5 cases embryotomy.

The above table is unavoidably incomplete; in the early part of my professional career, I carefully recorded all my difficult obstetrical cases, leaving the simple labors to fare for themselves; hence the reader will find an aggregate of 473 cases only, when, in reality, I am confident I have attended six hundred or more. This table exhibits all my recorded cases of every description, ranging from the seventh to the ninth month. It will be seen that I have transcended some obstetricians in the comparative frequency of some forms of difficult labor, but this may find an explanation, (at least in some degree,) in the fact alluded to above, that all the *difficult* labors are recorded, while the simple list is not replete. But amid all this freak of negligence and confusion, I have prepared a tabular numerical and per centage view of my experience, which I am inclined to think will successfully compare with results of some older tokologists, who have a larger experience, and more reputation than myself. If I am correct in this opinion I trust it will give an impetus to Southern Obstetric Medicine, and bring to light many facts of tokological importance, hitherto thrown aside as worthless and unimportant. If this desideratum should be attained, my highest hopes and expectations will be fully consummated.

For the sake of convenience, and the better understanding of the tabular statement, I have arranged the various classes and species of labor in *aggregate and distinctive forms.*

Aggregate view, No. 1.

		Frequency.	Per cent.
Natural Labor, - - -	5 cases twins,	1 in 94,*	or 1.05
Unnatural Labor, - - -	8 cases,	1 in 59,	or 1.69
Complex Labor, - - -	21 cases,	1 in 22,	or 4.45
Manual Labor, - - -	3 cases,	1 in 157,	or 0.63
Instrument. Labor - - -	7 cases,	1 in 67,	or 1.06

Distinctive view, No. 2.

EUTOCIA.

Presentations.	No.	Numerical frequency.
Vertex,† - - - - - -	429	429 in 473
Feet,† - - - - - -	3	1 in 157
Breech, - - - - - -	4	1 in 118
Face, - - - . - -	1	1 in 473

DYSTOCIA.

	Presentation, etc.	No. of cases.	Frequency.
Complicated, &c.	Arm and shoulder, - -	3	1 in 157
	Arm, foot and placenta, -	1	1 in 473
	Placenta previa, - -	2	1 in 236
	Side and funis, - - -	1	1 in 473
	Convulsions, - - -	4	1 in 118
	Tumor, - - - -	1	1 in 473
	Labial effusion, - - -	1	1 in 473
	Hemorrhage, - - -	2	1 in 236
	Adherent placenta, - -	1	1 in 473
	Hour glass contraction, -	3	1 in 157
	Cord torn from placenta in utero,	1	1 in 473
	Impacted head, - - -	1	1 in 473
Manual,—Cases required version, - - -		3	1 in 157
Instrumental,	Forceps, - - - -	2	1 in 236
	Required embryotomy or cephalotomy,	5	1 in 94

From the aggregate view No. 1, it will be seen that twin cases occur here in the ratio of 1 in 94, or 1.05 per cent. Unnatural labor, including face, feet, and breech presentations occur in the ratio of 1 in 59, or 1.69 per cent. Complex labor, including all *mal-positions, accidental or unforeseen deviations*, occur 1 in 22, or 4.45 per cent. Manual labor, including *turning only*, occurs 1 in 157, or 0.63 per cent., and instrumental labor, including *forceps*, *embryotomy* and *cephalotomy*, 1 in 67, or 1.06 per cent. It will be remembered that the list of simple eutocia is not

*A general average with Clarke, Boivin and Baudelocque.
† Twin cases not included.

replete; but let us contrast this picture with other authorities, and see how the case stands; and here, permit me to remark, the comparison is instituted with no invidious design, but merely to illustrate the position, and define the standard, as far as compatible, of Southern obstetricy.

Tabular view of relative frequency of various forms of Labor in French and English Practice.

Presentation, &c.	Baudelocque.	Boivin.	Bland.*	Collins.	Merriman.
Feet,	1 in 81	Uncertain	1 in 105	1 in 131	1 in 76
Breech,	1 in 59	"	1 in 52	1 in 40	1 in 00
Face,	1 in 296	"	Not known	1 in 504	Not rep
Arm & shoulder,	1 in 336	"	1 in 210	1 in 416	1 in 155
Placental,	Not rep.	"	Not rep.	1 in 1514	Not rep.
Convulsions,	"	"	†1 in 210	†1 in 693	1 in 105†
Hemmorrhage,	"	"	Uncertain	†1 in 555	1 in 558†
Twins,	1 in 91	1 in 132	Not rep.	1 in 69	1 in 76
Delivered by art,	1 in 76	Not rep.	"	Not rep.	Not rep.

By a brief recapitulation of the preceding tables, it will be seen that the following facts are demonstrated: 1. That we do not have unnatural labors as frequently as the French and English, as far as our tables are capable of determining. 2d. That arm presentations have occurred more frequently to me, than to Baudelocque, Bland, or Collins. 3d. That puerperal convulsions have occurred oftener with me than with Collins or Merriman, although I have previously declared that our ladies enjoy almost an immunity, and attempted to account for it; this unique position will find some atonement by recurring to Dr. Bland's experience in 1897 cases; and it will be fully explained in an annotation at another place. 4th. It will be seen that a lamentable deficiency exists in the tables of Boivin and Baudelocque, involving points of the highest practical and statistical magnitude. 5th. The mortality among the French women will strike forcibly the most casual observer, when it is remembered that the forceps are seldom used, and the perforation sacrilegiously interdicted.

But to place our own practice and that of others in their proper light, before the profession, it is necessary that we resort to another tabular history, showing the precise relative frequency of *mortality* and *instrumental labor*. By this table we are willing to be governed, and to award to every man his just position in obstetrical practice.

* Bland says 1 in 44 of his cases were difficult.
† These are transposed by oversight, and should be read so through the whole.

Table of Mortality and Instrumental Labor.

Accoucheurs.	Mortality.	Instrumental.	No. of cases.
Author,	None,	1 in 67	473
Baudelocque,	1 in 24	Uncertain	17,308
Boivin,	No report	1 in 183	20,517
Merriman,	1 in 210	1 in 98	2,947
Bland,	1 in 274	1 in 158	1,897
Collins,	1 in 156	1 in 114	16,654
Ritgen,	No report	1 in 9	103
Kluge,	"	1 in 15	1,111
Carus,	"	1 in 13	2,549
Minden,	"	1 in 12	295
Andree,	"	1 in 35	356
Kustner,	"	1 in 36	368
Boer,	"	1 in 96	9,589
Cusack,	"	1 in 34	1,268
Granville,	"	1 in 80	640
Seibold,	"	1 in 9	340
Voigtel,	"	1 in 5	29
Naegele,	"	1 in 28	1,711
Clarke,	"	1 in 162	10,199

The foregoing table exemplifies a remarkable exemption from mortality among lying-in women in this country, and we opine we hazard nothing in saying, that death in child-bed is an event of *uncommon* occurrence in Georgia. That some die, it would be folly to deny; deaths occur in every land, and in all circumstances and conditions of life. The facts in reference to instrumental labor are not so favorable as we could wish, but they can be plausibly and easily explained. 1st. The forceps cases, which are two, occurred both in the same woman, for a lateral pelvic deformity. This cause will continue to exist, and render her amenable to forceps deliveries at every lying-in term. This was irretrievable, and would have occurred to Collins, Baudelocque, Clarke, Boivin or any one else. 2d. The embryotomy and cephalotomy cases supervened under the following circumstances: 1st case. This was a case of impacted head, occurring with a young negress æt. 14; the head was firmly and immovably fixed between the sacrum and pubis; the patient had been in labor many hours, and was rapidly sinking; the child being probably dead, as no pulsation could be detected, I performed cephalotomy; the negress did well, but has never conceived since. Case 2d was a case of arm and shoulder presentation. The woman was a delicate female æt. 38; she was attacked with hemorrhage preceding labor on the day prior to my

being called; she was under the care of a *midwife.* I found the arm and shoulder presenting, with a prolapsus of the funis; the throes were violent, and the patient quite exhausted; an attempt at turning was made and abandoned, owing to the death of the child, and the risk incurred of rupturing the uterus. At the suggestion of Dr. Dill, embryotomy was performed; the patient did well. Cases 3d and 4th both occurred in the same individual, and were dependent upon the same causes,—pelvic *narrowness laterally*, and *uterine inertia.* This woman will never give birth to a living child of anything like ordinary dimensions. Every effort was made by myself and others, to rescue these fœtuses from the knife, but without avail: an imperative necessity, essential to the life of the mother, alone induced the operations. Cephalotomy is a painful resort, rendered doubly so where we have any reason to think the child is living, and should never be performed only from the most urgent necessity, and that, too, after due counsel with other practitioners. It is painful enough to perform such an operation, when there is reason to believe the fœtus dead, and I do not envy the feelings or reputation of any man who will plunge his knife into the brain of an innocent babe in utero, without sufficient cause, and after mature, calm and deliberate reflection, in conference with his brethren; yet there are those, who have but little patience, and are anxious to have the opportunity of securing an operative reputation, who will operate without just or sufficient grounds. Happily for women and children, their number is diminutive. It is probable these cases might have been remedied by a resort to the Cesarian operation. An important obstetrical question arises here, as to the validity of that operation. By the Cesarian operation, a risk is incurred of losing both mother and child, while by cephalotomy, *only one is sacrificed.* This is an important item, involving a great moral responsibility. It is the rule in English and American practice to sacrifice the life of the child; in this I fully concur, for reasons which are self evident, and founded upon moral and professional principles. In no event could I be induced to run the risk of sacrificing the life of a mother to save an infant, who in reality has no existence in the external world, but who, whatever may be its fate, must find a resting place in the bosom of the God who gave it. The 5th case was induced by a disproportionate head. This

was one of those unavoidable cases which cannot be remedied by manual interference; in consultation, it was resolved to operate, as the only means of saving the mother. This woman had ever had laborious and difficult labor; she was not capacious in her pelvic developements; the proportions of the child were very large; in no event could it have passed the pelvic straits without a diminished head. This closes an account of my cases of instrumental labor. Of the reasons which induced me to operate I shall not speak more definitely than I have, but leave the reader to his own conclusions, with the simple remark, "*that men are fallible, and often differ in their views of right and propriety.*"

Turning.—Version in obstetrics is one of those operations which it is easier to talk about than perform. The young obstetrician, who has listened to the declamations of a learned Professor for three or four winters upon the science of midwifery, will find himself quite chagrined at the first introduction he has to a case of several hours standing, requiring turning; ten to one, unless he has great manual dexterity, mixed with a good degree of self-possession and confidence, he will fail. Turning is usually performed by bringing down the feet, but it may be done by the head. In whatever way it is performed, the operator must exercise great caution and care; he must be patient, and remember he is not feeling in a *barrel*, or turning a log of wood about; the least error or undue force, may cost him his reputation for life. I have never performed the operation of version except in arm and side presentations; in one instance the child was saved, the others were lost.

I have usually, where the uterus was violently contracted, administered a full dose of opium before commencing the operation; and after its completion, if the pains were inefficient, I have ordered an infusion of ergot invariably, with the effect of having my anticipations fully realized. It is not the province of this communication to give general directions for the performing of these operations; indeed they can only be learned by careful experience in clinical instruction; I will therefore proceed.

Convulsions.—Probably no part of obstetrics is attended with more thrilling interest and intense anxiety than convulsive dystocia. Fortunately, as I have previously remarked, this form of labor seldom occurs among us. The four cases which came under

my care, were in three instances in the same person, and of the epileptic variety. The lady is æt. 35; she has had seven births. With her first and every subsequent child she has had puerperal convulsions; she is very subject to premature labor, which is always announced by a convulsion. The convulsions cease as soon as delivery ensues, but prior to that, they resist every therapeutic application for their suspension. I have seen them adopt a strictly tertian type with her, and be partially controlled by quinine; they invariably continue to, and through labor; and I never saw her have one after.

The last attack was in August, '49. I delivered her then of a fœtus at 6 months and 22 days; it is now living, and is a stout, fat child. After these attacks, she has speedy gettings up, and has a return of fine health until her next pregnancy.

The 4th case was a stout, athletic woman, with her first child; the convulsions are of an apoplectic order, unquestionably promoted by an overloaded stomach. This case yielded to *copious venesection*, *active catharsis*, and *delivery*. As a general rule, not invariable, however, the convulsions cease as soon as the birth of the child takes place, and the physician should be assiduous in his attentions, or the fœtus will be expelled before he is aware of it. These cases usually excite the mind a great deal, and often derange the imagination; causing many painful emotions. I recollect this last case was attacked on Monday, and after convalescence, she experienced a mental horror at the approach of Monday, and in several instances I was sent for to ward off the attack she expected on that day. These are the only cases I have seen prior to parturition, and I know of no physician here who has had a case, although I have made frequent enquiries. It is a protean disease, requiring the promptest attention, and the best of skill.

Placenta Previa.—I have met with but two cases of placenta previa; in both of these it was attached partially to the os uteri; they both occurred at the full period. I delivered in both cases the placenta first, and as the pains were violent, the children in both were soon expelled. I am induced to believe that the danger in such cases does not originate from the placental presentation, or it would cease to exist as soon as it was expelled, but it is derived from the exposed orifices of the uterine vessels. It is very clear to my mind that this is correct, from the fact that the placenta is

usually detached in the beginning of *labor*, consequently the woman could not suffer from placental hemorrhage, as no affinity exists after that time with her and the child through the placental circulation. Now it is undeniably correct, that in placental presentations, the attachment being about the neck of the uterus, the flooding would be more violent from the exposed orifices, owing to the violent contractions of the body of the womb, producing an expulsion of the fœtus, and a necessary opening of the os uteri. This is a plain but truthful process, and to me fully demonstrates the theory of hemorrhage dependent upon placental presentations. If you deliver the placenta in these cases, the flooding does not cease, but if you deliver both child and placenta it will. We presume no man of any experience will doubt this position; and what does it prove? It surely establishes the point I have taken,—*that placental hemorrhages (as they are called,) depend upon an exposition of the mouths of the uterine vessels, not upon an adherence of the placenta to the os uteri.* What then is the practice? It is *plain, self-evident, and unquestionable; deliver the placenta, promote uterine contraction, and deliver the fœtus as soon as necessary, and with any means your judgment may dictate.*

Hemorrhage.—Hemorrhage during pregnancy is not an uncommon occurrence. It is often alarming, requiring the most decided means to stay its progress; when again it is of little importance, requiring nothing more than quietude and the recumbent posture to restore the patient. From the 7th to the 9th month, I have met but two cases; they were mild in their character, and demanded nothing unusual. The emergency of such cases always depends upon the quantity of blood lost and losing, and the ability of the patient to bear it. The points of practice are always to be determined in reference to these facts. I have never had a case of uterine hemorrhage, where I gave ergot prior to delivery.

Hour Glass Contraction.—This is dependent upon a circular contraction of the uterus at the point where it exists, and is often a source of considerable annoyance to the accoucheur, particularly if called late. If you are present, the difficulty is usually easily overcome by a gentle introduction of the hand and fingers into the point of constriction, and gently extracting the placenta; if a failure should happen, patience is a fine remedial agent; should all these fail, bleed and give opium. I have never had much difficulty in

these cases, but can easily imagine that they can be very annoying. Always bear in mind gentleness is the hand-maid of skill in these cases. Some seem predisposed to hour glass contraction.

Adherent Placenta.—This is a perplexing and often difficult complication in obstetrical practice. An adherent placenta is really an untoward event. Fortunately for females, it does not very often exist. Physicians are very frequently called to patients with the belief that they have adhering placentas; this is generally an error of judgment, for in a large majority of cases it is only a retention of the mass. These conditions are widely different and are not to be identified as one in obstetrical practice. Adhesions of the placenta are usually schirrous, cartilaginous, or ossified, and they are to be extracted with great care and gentleness; indeed in all manipulations in the inner womb,"*be gentle,*" should be our motto; *violence is "death."* The hand should be well oiled, and easily introduced, when the placenta should be carefully, cautiously and quietly detached from the uterus. If a portion should remain and cannot be extracted, it will be decomposed and come off; in this case we should use tepid antiseptic enemata and washes daily.

In retention of the placenta, the same caution should be observed. In my own practice I never have a retained placenta. I invariably deliver in 15 minutes after delivery, all things being fair. I have never had any cause to regret the practice, but the experience of every week proves to me its correctness. It is seldom I see a hemorrhage after delivery among my own cases, and I attribute it to the speedy delivery of the after-birth.

Labor, Duration, Spontaneous Evolution, and anomalous forms.—Labor is that process by which the child is expelled from the genitals of the woman; it has many varieties, as has been seen, and which it is not necessary to reiterate. Labor may be retarded by several causes, which are laid down in the books; but one of the most common causes of retardation in simple eutocia that I have met with, is the cord being around the neck of the child, and it is much more difficult to remedy than a tyro would suspect. Labor is generally accompanied with pain; but it it not an invariable attendant. I remember to have seen a negress bring forth a fine large child, without the least semblance of pain evident to myself or others; indeed she positively denied having the child until it

was exhibited. The duration of labor is a pleasing and interesting question; it is not settled, and will not probably be for many obvious reasons. In a country practice many circumstances conspire to prevent physicians from keeping a record of facts in reference to this subject; it is seldom we are called at the commencement of labor, which accounts at once, in a great degree, for our inability to keep a table; but in my own practice, as far as I have been able to observe, the duration of simple labor does not exceed six hours. The doctrine of spontaneous evolutions in transverse and brachial presentations, was first taught, we believe, by Denman; since which time it has been verified in numerous instances by many accoucheurs; at the present day it is not a problem. I met with a single instance of this evolution; it differed in no respect from other cases of the kind; the child was dead, which is usual in all such cases. I should deem it a rare event for one to be born alive.

In obstetricy the practitioner will meet with many deviations of presentation, which are mentioned by authors; in all such he must be governed by his own superior judgment, and general principles. I have met a case of twins, where one was born in the early part of the day, the other was delayed until the next evening—nearly 36 hours; in this case I was guided alone by general principles; the patient had no *pain or flooding—she was calm and quiet.* I waited patiently; the other child came along in proper order, was sound and healthy; the woman did well. In twins, authors tell us, there is a great proclivity to flooding; I have not found it so; my twin cases do as well as any others. I saw a case in which the cord was torn from the placenta in utero; being present early, I immediately introduced my hand, and brought away the after birth, which had been retained. These errors are remedied with facility, when we are present soon after their occurrence, and the practitioner has the essential ingredients of a skilful tokologist—*self possession and confidence in his ability*; without these, success in every department is equivocal. In two instances labor was impeded by the vulva being effused with serum and blood. The first was punctured, and the case relieved in due season; the second was originally a *tumor*, it became pulsative from an injury. I plunged my lancet into it; the dark grumous and offensive matter it contained was discharged; the labor was

soon after completed by the natural efforts. The other cases which came under my inspection, merit no special attention, and I shall not advert to them.

Ergot.—Much has been said in justification and denunciation of this drug; by one it is represented as dangerous to the child; by another it is denounced as producing hour glass contractions; and by a third it is proscribed for inefficiency. I have used ergot in almost every form of labor, and I can accord to it none of these properties. In my hands, it has ever been prompt and efficient; I never saw any injury from it in any stage of labor. I believe it excites uterine contractions and promotes dilatation of the *os uteri.* I have never had a case of hemorrhage after giving it prior to delivery. I believe it almost a specific in those cases where there is a tendency to flooding; it should be given 20 or 25 minutes before labor is consummated. The only error it is guilty of, is a predisposition to operate upon the bowels in some women. This can be modified by giving it in conjunction with a little paregoric.

Pregnancy.—Hippocrates, Galen, Pliny, La Motte, Haller, Aristotle, Petit, and Levret, contended that pregnancy terminated at the end of nine calendar months, but might be prolonged to ten or fifteen. Now I am not sufficiently versed in gestation to definitively determine this point, but in all the cases I have witnessed in reference to this question, pregnancy has terminated at about the 39th week, plus one day. The quickening period is about the 4th month; five months from this parturition will usually take place. I think the period of gestation is subject to slight changes; it may be a few days over 39 weeks, or a few under it. There is great discrepancy among women in reckoning, and in no instance ought the count of the patient to be strictly relied upon, unless it be with a lady remarkable for her exactness and perception in such things. I remember the case of a lady who brought forth precisely 39 weeks and 2 days from the day of her marriage. I know another instance in which the woman was confined precisely 39 weeks, one day, from the return of her husband who had been gone for four months.

While on the subject of pregnancy, I will remark, that I am inclined to believe in the hereditary influence of labor so far as *quickness, or tediousness of the process is concerned* In a large majority of cases, I have found where women had quick labors,

their mothers before them had also, and vice versa. Now I will not assert the rule as invariable, but it is very apt to be the case.

I have now gone through what I fully intended when I began—*a synoptical account of my obstetrical experience.* I have extended the paper much farther than I at first intended to do. I hope, however, its unforeseen prolixity will not be at all detrimental. It is possible in some parts of its serpentine course, I may have committed some errors, if so, they are unintentional and will be pleasurably rectified. I have not thought proper to pursue a strictly systematic course in treating of the various topics involved, for the reason that the paper was intended as a practical effort, with no didactic intention, but only as my own experience. In conclusion, I will make a small tabular view of facts and observations in reference to general items.

Miscellaneous Table.

Duration of labor, 6 hours.
Duration of pregnancy, 39 weeks, 1 day.
1 Case spontaneous evolution.
1 Case 36 hours between births.
1 Case without pain in labor.
1 Case child with teeth.
1 Case imperforate anus.
1 Case hydatids, (*not mentioned in tables.*)
4 Cases still born, *restored.*
2 Forceps cases, both children living.
In a majority of all the cases, males predominated to a small extent.

The Treatment of Acute Rheumatism, illustrated by cases taken from private practice. By Geo. L. Upshur, M.D., Norfolk, Va., Member of the American Medical Association.

Case 1.—Mrs. W., aged 30, of sanguineous temperament, and rather delicate, was seized with pain in the right shoulder joint on 23d of September, 1846. The weather had been inclement for several days previous, and she had imprudently exposed herself. I saw her on the 24th, at which time all the large joints were somewhat painful, although there was little or no fever.—*Prescription.* ℞. Pulv. Dover. gr. xv. Div. in chart. 3. S. one powder every three hours.

25th. Patient much more uncomfortable; slept none last night; nearly every joint red, swollen, and exquisitely tender; pulse, full, hard, and bounding; bowels confined; tongue foul, thirst

urgent, and perspiration very profuse. Being desirous of avoiding, if possible, the use of the lancet in so delicate a subject, I ordered only the following: ℞. Vin. Colch. Rad. f.℥iss.; Magnes. calc. ʒij.; Magnes. Sulph. ℥ss.; aquæ f.℥iv. M. S. A table-spoonful every three hours, until it purges freely. At 9 o'clock P. M. the mixture had produced several copious stools, but without relief to any of the symptoms; took 20 ounces of blood from the arm,

26th. Symptoms somewhat improved; less fulness of the pulse, much less perspiration, and not so much pain. ℞. Quiniæ Sulph. ʒss. Div. in chart. 3. S. One powder every three hours.

On the 27th, my patient was so entirely relieved, that I considered her convalescing, and ceased to visit her after the 28th.

Case 2.—Miss B., aged 19, very large and robust, was taken with chill followed by high fever, with metastatic pains in the joints and a profuse warm perspiration, on 14th of January 1847. Sent for me in the night, and I have never seen a better marked case of acute articular rheumatism—even the joints of the fingers and toes were red and swollen. The blood was fairly running riot through the vessels, and the patient alternately shrieked and ground her teeth in very agony. I bled her to full 25 ounces, which checked the perspiration, and lessened the pain in a considerable degree; ordered gr. x. of Dover's powder to be taken at once.

15th. Somewhat more comfortable; still suffering very much. ℞. Ol. Tiglii gtt. ij.; Saponis Hisp. q. s. M. Pil. 2. S. Give one pill immediately, and if it does not act in four hours, the other.

16th. Took both pills which acted copiously; passed a comparatively good night; and is decidedly better this morning; pills vomited her freely; ℞. Calomel gr. x. Ol. Tiglii. gtt. iss. M. Pil. 3. S. one pill every hour.

On the 17th the patient was on the verge of convalescence; ordered ten grains of Dover's powder at bed-time.

18th. Improving; opened the bowels with croton oil.

19th. Pain nearly subsided; no fever; appetite returning. Ordered a table-spoonful, three times a day, of the colchicum mixture, to keep the bowels soluble. On the 24th discharged her cured.

Case 3.—Mr. E., aged 22, sanguineous temperament, was seized with acute articular rheumatism, May 16th, 1847; saw him on 17th; pain confined chiefly to shoulders, elbows, and wrists, which were red and swollen. Prescribed croton oil in purgative doses followed by the colchicum, magnesia, &c. as in other cases. Discharged him convalescent on the 19th. On the the 6th of August following he was again attacked, and again relieved, in a great degree, by the croton oil. The disease, however, became chronic in the course of two or three weeks, when I gave him the vinum colchici, and subsequently the iodide of potassium with the fluid extract of sarsaparilla, with fine effect; I also derived great benefit from "firing" around the joints as recommended by Dr. Corrigan in the *Dublin Hospital Gazette* for March 1846. The article is too long to insert here, but it may be found, condensed, in vol. 4th of Ranking's Abstract, p. 102, and is well worth a perusal.

About four weeks after the disease assumed the chronic form, there appeared upon the extremities, a pustular eruption, resembling very closely, the eruption of small pox. The base of the pustules, however, instead of being red as in variola, was livid and variegated, putting one in mind of the blotches seen in secondary syphilis. I have no doubt but that there was a syphilitic taint in this case. After two or three months, Mr. E. returned to his friends in New England, much improved, but not entirely relieved. I have not heard whether he ever permanently recovered.

Case 4.—Mrs. Wiles, a very delicate lady, aged 25, mother of two children, and withal a confirmed dyspeptic, was taken sick with acute rheumatism in its severest form, May 29th, 1847. When I saw her on the 30th, her fever was high, the pulse frequent without fulness, perspiration profuse, bowels confined, and pain in the joints unbearable. Purged her violently with croton oil, and on the next day, 31st, gave ʒi. of Pulv. Jal. Comp., which acted well.

June 1st. Much better; fever, pain and perspiration greatly lessened; determined to give the colchicum a fair trial. I gave it to her without intermission, until the 8th, but apparently without benefit; I then prescribed quinine, ten grains three times a day;

she took in all sixty grains, and was discharged cured on the 12th of June, having been ill just two weeks.

Case 5.—Miss M., aged 30, robust, was seized March 28th, 1848, with chill. When I saw her, all her joints were involved, and there was scarcely a voluntary muscle in the body that could be moved without pain; fever high, abundant perspiration, and constipated bowels; ℞. Ol. Tiglii gtt. vi.; Calomel gr. xxx. M. Pil. 6. ℞. Vin. Colch. R. f.ʒi.; Magnes. Calc. ʒij.; Magnes. Sulph. ʒiij.; Aqua f.ℨiv. M. I directed her to take one of the pills immediately, and after it ceased to operate, to begin with the mixture and take a table-spoonful every three hours. On visiting her next morning, I was alarmed at her condition. She was excessively feeble, pallid, with her extremities cold and shrivelled, and passing blood from the bowels every ten minutes; each stool being accompanied with violent straining and tenesmus. The whole abdomen was tender upon pressure; with nausea, and a frequent, feeble pulse. *With all this, however, she had not even a remnant of the rheumatism, and could move her limbs about in any position without the slightest inconvenience.*

Upon inquiry, I ascertained that the nurse, a listless, inattentive creature, had mistaken my directions entirely, by giving the *pills* as I ordered her to give the *mixture*, namely, one pill every three hours. The patient took six drops of croton oil, and thirty grains of calomel, within eighteen hours, and, as a matter of course, was purged nearly to death. The only wonder is, that she did not die before I saw her. She recovered slowly from the effects of the hypercatharsis, and has not had a rheumatic pain from that day to this!

Case 6.—Frances B., aged 12, lymphatic temperament, seized Sept. 23d, 1848, with well marked acute articular rheumatism; the upper joints chiefly affected, but the disease showing a decided tendency towards metastasis. Prescribed Ol. Tiglii gtt. i.; Calomel gr. v. M. S. To be taken immediately. The following day there was a decided amelioration of all the symptoms. ℞. Quiniæ Sulph. gr. xxx; Div. in chart. 4. One powder to be given every two hours. The patient was well enough to sit up on the 26th, when I ceased to visit her.

Case 7.—On the 3d of October, 1848, I was called to see Mr. W., aged 46. He had been in feeble health for a year or two before, and was then suffering from a chronic ulcer upon the left leg. For several days he had been troubled with sharp rheumatic pains in the shoulders and back of the neck. He had very little fever; bowels confined. Gave him a table-spoonful of the colchicum mixture every three hours. The next day he was rather worse, when I purged him with croton oil. On the 6th, purged him again with the same, and again on the 8th, and on the 9th I found him entirely relieved. In a subsequent attack, I gave him the decoction of may-apple (Podophyllum) which acted copiously on the bowels, and gave him great relief.

Case 8.—Saw William S., aged 40, October 11th, 1848; fever, constipated bowels, and pain, redness and swelling of all the large joints. Prescribed Ol. Tiglii gtt. ij., to be taken at once; repeated the dose the following day, and on the 13th found him so much relieved that he required no further treatment.

Case 9.—Mr. Hubert S., an athletic young man of sanguineous temperament, was seized Nov. 28th, 1848. Three years before, he had an attack of rheumatism, which confined him to the bed four weeks. When I saw him, the ankle and knee joints only were very painful, with now and then a twinge in the shoulders. He had but little fever, and no perspiration. Prescribed the croton oil as in the other cases, and repeated the dose on the next day. The symptoms were relieved entirely, with the exception of slight pain and stiffness in the right ankle joint. Ordered 5grs. of iodide of potassium, to be taken four times a day in infusion of hops; under this treatment he recovered in about ten days.

Case 10.—Mr. John M., was taken violently on the 8th of December, 1848. He was a fleshy, muscular man, and suffered intensely. Gave him the croton oil as usual, followed by eight grains of quinine every hour, until he had taken 40 grains. He was entirely relieved on the 10th, and had no return of the disease.

Case 11.—Mrs. F., aged 26, recently marrried, anemic and

and feeble, was taken sick on the 18th of January, 1850. Pain, metastatic, but confined chiefly to the upper joints. She was purged freely with croton oil every day, until the 21st, when I prescribed the following. ℞. Mass. Hydrarg. gr. xx. Quiniæ Sulph. ʒss.; M. Pil. 6. S. One pill every two hours. On the 24th purged her again with croton oil, and discharged her cured on the 26th.

Case 12.—Mrs. Fanny ——, aged 35, had been suffering from dyspepsia for more than a year. About ten days before I saw her, which was on the 18th of March, 1850, she felt considerable pain in the shoulders, back of the neck, and sides of the chest. Her bowels were confined, and she had some febrile movement. I gave her alternately, every day, a drop of croton oil, and fifteen grains of quinine, for four days. She then took ten grains of Dover's powder every night, followed by a drop of croton oil in the morning, until the 24th of March, when she was discharged, cured.

Remarks.—A celebrated physician being once asked what he considered the most successful treatment of acute rheumatism, answered, "six weeks"—thereby plainly intimating his opinion of the intractable nature of the disease. While I am ready to admit that there is *some* truth in this opinion, and that rheumatism now and then assumes a form which might well entitle it to a place among the *opprobria medicorum*, I am not at all prepared to entertain the notion, that early and judicious treatment has no power, in a majority of cases, to alleviate its pangs, or shorten its duration. On the contrary, I believe that rheumatism is as amenable to proper management as any disease of equal severity.

Within the past five years, I have accurately noted the history and treatment of about *thirty* cases of acute articular rheumatism, from among which the twelve cases just detailed are indiscriminately taken. In these, it will be seen, the average duration of the disease, was 5½ days; the longest being 14, and the shortest 2 days. *Croton oil* was used in 11, *colchicum* in 6, *quinine* in 6, *bloodletting* in 2, and *iodide of potassium* in 2. I desire to say a word or two upon each of these agents.

Of *croton oil* as a purgative in acute rheumatism, I am prepared

to speak in the highest terms. Cathartics have always stood in the front rank of remedies in this disease, but I am disposed to believe that the efficacy of croton oil does not depend entirely upon its cathartic properties; it possesses a power over the disease beyond these, and apparently not dependent upon them, for other cathartics, which act as powerfully and as promptly, producing similar watery stools, do not bring a like amount of relief to the patient. I do not say that it is a *specific*, for I am not a believer in the doctrine of specifics, in medicine; that doctrine has put more stumbling blocks in the way of medical progress than all the open quackery of the past half century. I merely desire to state, that after a fair trial, in a number of cases accurately observed, where there was scarcely a possibility of falling into error, I believe that the croton oil is the *best* single remedy in the treatment of acute rheumatism; and I am thoroughly convinced, that it is as justly entitled to the term *specific*, in this disease, as is quinine in miasmatic fever. I speak now, not of chronic rheumatism, nor of that which results from a syphilitic taint, but of the acute inflammatory affection. In all the eleven cases, in which it was given, its beneficial effect was palpably evident, but particularly as in cases 5, 6, 8, 10, 11, and 12.

Dr. Wm. A. Thom, an intelligent physician of Northampton County, to whom I am indebted for the first hint in regard to the value of croton oil in this disease, informed me, about two years ago, that he had used it in several severe cases, and uniformly with success.

Colchicum has uniformly disappointed me. In cases 1, 4, 7, it was fairly tried, and although it invariably acted freely upon the bowels, there was no alleviation of the distressing symptoms. In the chronic disease, I think I have sometimes derived great benefit from it, but in the acute form, *never*. It may be, that I have been so unfortunate as always to get hold of an inferior preparation, but some how or other, I have taken the notion, that when a patient is treated with colchicum *only*, he would do just as well to take old Dr. Warren's *six weeks* instead.

Quinine I would place next to croton oil in the treatment of rheumatism. That it is decidedly *sedative* in large doses, is undeniable, and therefore it may be exhibited during the febrile stage. Given after the use of the lancet, in highly inflammatory cases, or

after powerful purgation with croton oil, it has been productive of the happiest effects in my hands. I never give to an adult a smaller dose than ten grains, repeated three or four times a day; less than this would be worse than useless. It is particularly serviceable in those cases which perspire most profusely.

It seems to me that *bloodletting* possesses no direct *curative* power in the treatment of rheumatism. In robust persons, with high fever, intense pain, and a full, bounding pulse, I think it subserves a useful purpose in rapidly subduing the over-excited circulation, thus placing the system in a better condition to respond to the impression of true curative agents. It is true that bloodletting is one of the most powerful antiphlogistics, and that rheumatism, in its acute form, presents undeniable evidences of its highly inflammatory character, nevertheless it has long since been voted an inflammation, *sui generis*, and I believe there is a great deal too much of the *neuralgie* about it to justify the heroic use of the lancet so warmly advocated by Bouillaud. *Topical* depletion has never been of the least service in my hands, except where the disease was confined to a single joint, *with no disposition toward metastasis.*

When the disease assumes the chronic form, or if there should exist a scrofulous, or syphilitic taint, no remedy will be found equal to the *iodide of potassium.* I usually order five grains four times a day, to be taken in hop tea, the bowels in the meantime being opened every day with the black draught, or other cathartic. I have rarely seen the most obstinate cases refuse to yield to this treatment.

In the second attack of Case 7, almost immediate relief followed copious purgation with the decoction of *may-apple* (podophyllum,) but it is very unphilosophical, particularly in so uncertain a science as Therapeutics, to deduce a general conclusion from a solitary observation, so I will not venture to recommend the may-apple until authorized by further trial. I have adverted to it, merely because it forms a part of the history of the case; and with the remark, that it is considered by all of the old *negro-doctors* in this section of country as a *specific* in rheumatism, I dismiss the subject.

Norfolk, Va., Sept. 5th, 1850.

Absorption of the Testes from Metastasis of Mumps. By COLUMBUS HIXSON, Student of Medicine, Middletown, Guernsey County, Ohio.

A. G., aged 25, in April, 1842, was attacked with mumps, which at that time were prevailing in an epidemic form in the neighborhood. The family physician was called, who prescribed the usual antiphlogistic treatment. In the course of three weeks the patient had so far recovered as to be able to walk about his chamber. The swelling of the parotid glands had entirely disappeared, and the patient thinking himself convalescent, made a visit upon horseback to the village about one and a half miles distant; and returned in about 3 hours, with slight sensations of pain in the testicles, which in the course of a few hours became so severe as to confine him to his bed. The physician was again called, who prescribed fomentations, a suspensory bandage, and the unguentum iodini comp. to the scrotum, and rubefacients to the parotids. Under this treatment the patient speedily recovered, the swelling was dispersed and he has ever since enjoyed excellent general health. Shortly after this occurrence, he noticed small tumors forming in the substance of the scrotum, which, at the present time are very numerous, but most abundant on the left side of the scrotum, and which at times run on to suppuration, and when punctured, some emit a fluid resembling healthy pus, while others discharged a fluid of the consistence of paste. The scrotum has also been attended with itching sensations, which at times is almost intolerable.

Shortly after his convalescence from the mumps, our patient noticed that his testicles were becoming smaller, which caused a great deal of uneasiness. Several medical gentlemen were consulted, but none gave him any encouragement respecting a cure. At last he disclosed the case to the writer, who, after examination, determined to report the case, hoping that it might elicit from the pen of some one an investigation. The testicles of this individual, at the present time, are not more than one third their original size, the right one is considerably smaller than the left.

From this history of the case, which is a correct one, there appears to have been a departure from the ordinary phenomena observed in the course of this disease. So far as the writer is aware it is the doctrine of pathologists, *that the testicles are liable to become*

implicated at the time of the swelling leaving the parotid glands. In this case it appears from the patient's own acknowledgments that *swelling or inflammation had entirely left its original seat,* and he had so far recovered as to consider himself convalescent. In the opinion of the writer, the tumors present in the scrotum, are but a morbid condition of the sebacious glands of that part. Should this article meet the eye of any medical gentleman, who feels himself competent to the task, it is hoped that the treatment most likely to succeed will be pointed out.

Case of death from fractured Femur. Reported by S. B. Mills, M. D., Resident Physician, City Hospital, San Francisco, California.

William Toppin, entered the city hospital of San Francisco, Feb. 29th, with fracture of the right femur, having received said injury in a fit of intoxication, by falling down a precipice at " Clark's point," and having been treated on board the U. S. ship of war Warren,—— Assist. Surg., for some months previous. At a consultation of the most eminent men of this place, and at the earnest solicitation of the patient, it was concluded prudent to operate, and consequently Dr. Peter Smith, city physician, operated, Jan. 19th, for false joint. The patient's bad condition of health made it impossible to hope for much good, but in a desperate case we employed a desperate remedy. The said William Toppin died June 20th.

On examination we found a transverse fracture with the ends of the bones dove-tailed; the ends of the bones remaining healthy with a very slight inflammation of the periosteum.

[The above case we deem of great importance to the profession. It shows how careful a surgeon should be in ascertaining the exact position of the fragments of fractured bones; and in their coaptation. In this case we understand, the former were widely separated by an interposing muscle; and if such were the fact, would it not have been better, even at the hazard of making a compound fracture, for the surgeon to cut down and remove the muscle from between the fragments, after all other resources had failed, rather than to wait, as was done, until so much callus had been thrown out; and the swelling of the thigh had so increased, that the condition of the fragments could not be ascertained ? G. R. B. H.]

BIBLIOGRAPHICAL NOTICES.

Surgical Anatomy. By Joseph Maclise, Surgeon. *With colored plates.* Part III. To be completed in four parts. Philadelphia: Lea & Blanchard: 1850.

The 1st and 2d parts of this work have already gained for it a high reputation as a treatise upon Surgical Anatomy: it is, therefore, unnecessary to give any detailed account of the contents of this part, in order that we may commend it to our readers. Every one who is conversant with the medical literature of our country, is conscious that there is an imperative demand for works of this class, and will congratulate the American public upon the appearance of Mr. Maclise's labors in their present form and dress.

The importance of this kind of knowledge, the difficulties under which it is usually obtained, and the facility with which it is forgotten, afford cogent reasons for pressing the usefulness and value of this work upon every student of medicine.

Although we cannot admit for one moment that there is any authority in Anatomy equal to the dead subject and the scalpel, and that for these there can never be a substitute, nevertheless, since so few appeal to this source of knowledge, and since (of these few) some approach it with preconceived notions from books; we cannot but rejoice that there are some sources of authority that are in accordance with nature and the truth. If a large number *will* teach Anatomy from books, and a still larger number learn it from the same source, we rejoice to know that Maclise's Surgical Anatomy will inculcate more truth and fewer errors than most of its predecessors.

This 3d part contains sixteen plates relative to Inguinal and Femoral Hernia, with instructive commentaries upon each. We hail those upon Inguinal Hernia with most pleasure, because we conceive that greater error exists among teachers and students upon this form of hernia than upon any other. Every one, whether an Anatomist or Surgeon, must recollect the time and labor expended upon this subject in the lecture-room, and the apparent paradoxes and contradictions with which it was surrounded. The very fact, that the subject of Hernia is a "pons asinorum" to students, is an evidence that there is an obscurity connected with its demonstration. The parts concerned are not so numerous or minute as those

of the eye or brain, and yet these latter are learned with comparatively little patience and trouble. Though the points to be studied in hernia are few in number, and can be seen without microscopes, the student generally leaves the lecture-room with a degree of perplexity which cannot in justice be attributed to the subject, and which is only removed by his own personal dissections.

This obscurity arises from the application of a defective nomenclature, a kind of adherence to the description given by some great authority, and in making dissections to *suit a description.*

Anatomical nomenclature probably never can be altered: we can never expect a perfect uniformity of terms, nor is it necessary that there should be, any more than that we should all speak the same language. But, it is extremely important that students should be taught something more than mere names, that they should not be confused with long Latin synonymes, before receiving clear ideas of facts: they should learn that terms are but conventional and representative, whether invented by Camper, Scarpa, or Sir Astley Cooper. These are the views and feelings of Mr. Maclise when he speaks of the difficulty and perplexity with which this impenetrable fog of surgical nomenclature besets the progress of the beginner.

We have no hesitation in saying that no one is able to make a proper demonstration of the anatomy of hernia, unless he can make an intelligent beginner clearly understand it without the use of technical terms.

How often has an hour been wasted in endeavoring to prove that Sir Astley Cooper called this or that the Cribriform Fascia; as if the great idea to be taught was, that the dissection presented to their view was in accordance with that of Sir Astley; as if the truth was not the same, no matter upon what plan the dissection is made, provided it is not forced by knife-handles or pointers.

How unfortunate would it be for a student if this fond adherence to terms should affect the prescribed text-books of any medical school; one with an air of confidence and certainty will say, this is the "cribriform fascia," another with a tenacity equal to the other's boldness, "that is the cribriform fascia; Sir Astley told me so."

> "Strange there should such a difference be
> Twixt tweedle dum and tweedle dee."

A sentiment which seems to have actuated Mr. Maclise in his work is thus expressed by him : "But when we view nature as she is, and not fashioned by the scalpel, we never fail to find an easy explanation of her form." This follows a description of the inferior edges of the internal oblique and transversalis muscles. Our readers may recall, among the dim memories of inguinal hernia, something, called the crural arch, and, perhaps, they may recollect seeing the same pictured forth with beautiful regularity, in some drawing or painting of the subject, about the lower part of the belly.

Now, Mr. Maclise says nothing about the crural arch in this connexion, and hence, therefore, some may not consider the work as orthodox ; especially as they *know* that Colles, or Scarpa, or Cloquet, or Lawrence, says there is a crural arch. This reference to authority and not to the dead subject, at once settles the matter in their minds. But let the reader peruse the following passage, and judge for himself.

"The arched inferior border of the transverse muscle, F, Plate 30, expresses by its abrupt termination that some part is wanting to it ; and this appearance, together with the fact that the fibres of this part of the muscle blend with those of the internal oblique and cremaster, and cannot be separated except by severing the connexion, at once, suggests the idea that the cremaster is a derivation from both of these muscles.

"Assuming this to be the case, therefore, it follows that when the dissector removes the cremaster from the space L *h*, he himself causes this vacancy in the muscular parietes of the groin to occur, and at the same time gives unnatural definition to the lower border of the transverse and oblique muscles. In a dissection so conducted, the cord is made to assume the variable positions which anatomists report it to have in respect to the neighboring muscles. But when we view nature as she is, and not as fashioned by the scalpel, we never fail to find an easy explanation of her form.

"In the fœtus, prior to the descent of the testicle, the cremaster muscle does not exist. (Cloquet. op. cit.) From this we infer, that those parts of the muscles, EF,* Plate 30, which at a subsequent period are converted into a cremaster, entirely occupy the space L *h*. In the adult body, where one of the testicles has been arrested in the inguinal canal, the muscles E F, do not present a defined arched margin, above the vacant space L *h* but are continued (as in the fœtus) as low down as the external abdominal ring. In the adult, where the testicle has descended to the scrotum, the cremaster exists, and is serially continous with the muscles E F, covering the space L *h* ; the meaning of which is, that the cremasteric parts of the

*Internal, oblique and transversalis.

muscles E F, cover this space. The name cremaster therefore must not cancel the fact that the fibres so named are parts of the muscles E F. Again, in the female devoid of a cremaster, the muscles E F present of their full quantities, having sustained no diminution of their bulk by the formation of a cremaster. But when an external inguinal hernia occurs in the female body, the bowel, during its descent, carries before it a cremasteric covering at the expense of the muscles E F, just in the same way as the testicle does in the fœtus. (Cloquet.)

"From the above-mentioned facts, viewed comparatively, it seems that the following inferences may be legitimately drawn :—1st, that the space L *h* does not naturally exist devoid of a muscular covering; for, in fact, the cremaster overlies this situation ; 2d, that the name cremaster is one given to the lower fibres of the internal oblique and transverse muscles which cover this space ; and 3d, that to separate the cremasteric elongation of these muscles, and then describe them as presenting a defined arched margin, an inch or two above Poupart's ligament, is an act as arbitrary on the part of the dissector as if he were to subdivide these muscles still more, and, while regarding the subdivisions as different structures, to give them names of different signification. When once we consent to regard the cremaster as constituted of the fibres originally proper to the muscles, E F, we then are led to the discovery of the true relations of the cord in respect to these muscles."

There is another point upon which we consider Mr. Maclise to have added much that is valuable to teachers and students, and this is expressed in plates 31 and 33, exhibiting the fascia transversalis as far as it relates to the anatomy of parts concerned in hernia, a point concerning which there is such a diversity of expression.

This diversity of opinion arises, as we have said already, from a want of reference to the dead subject, and a dependence upon authority for truths which we should acquire or make our own by study. Let the reader refer for instance to the plate of Sir A. Cooper exhibiting his fascia transversalis, and that which is called the internal abdominal ring, and he will there see represented a dissection which is well calculated to give a most erroneous idea, and which is a most fruitful source of error on this subject. There is pictured an oval hole in the fascia which was made by the dissector, and which is understood by the ignorant to be so in nature, which Sir A. C. never meant to teach. In this point and in many others we consider Mr. Maclise's drawing far superior to any others, for the simple reason that it represents the truth.

The "internal spermatic fascia," (Cooper) and "infundibuliform fascia," (Cloquet) is shown to be continuous with, but a prolongation of, the fascia transversalis. This is not only true in nature where no hernia exists, but also where inguinal hernia does occur, unless the case is an exception, and the hernia is the result of force and the tissue actually ruptured. If the work of Mr. Maclise be read and studied by those who cannot or will not read nature, they cannot fail to get rid of the erroneous impressions which result from early education, and learn that the internal abdominal ring is not a hole or fenestrum.

The plates illustrating the descent of the testicle are equally well calculated to teach the truth as it is in nature, and the laws by which she is governed. They show how the testicle descends, pushing before it, and enveloping itself with, every lamina which constitutes the parietes of the abdomen; the modifications which these laminæ undergo in becoming the investments of the cord and testicle; and that in every oblique inguinal hernia, the bowel pursues the same course.

The work is equally satisfactory with reference to Femoral Hernia. The plate exhibiting the crural ring and the description, the descent of the bowel and the manner in which it receives its coverings, are shown in a clear and instructive manner; and we have no doubt that many will in their hearts confess that this work of Mr. Maclise has taught them what they never knew before concerning the anatomy of hernia.

How simple does the subject of hernia become when treated of by a philosophical anatomist, who is not only familiar with facts and details, but with the causes which produced them! How indelible is the truth acquired by reason compared with that acquired by memory. When in the first place we are taught the true anatomy of the parietes of the abdomen in general, and then the descent of the testicle, and the particular relation of these parietes to the cord and testicle, and that when the bowel descends, it follows the course of the cord (in oblique inguinal hernia,) and that whatever is true of the cord and its coverings, is true of the protruded bowel,—we then distinctly see the reason of things, and the connection of one fact with another, that *rupture is not a rupture;* that this term is calculated to give rise to the idea of holes with defined edges;

whereas, there is no such thing in nature, not even at the external abdominal ring; and whenever such an opening is presented to us, we recognize it at once as the result of an injury, or as a false dissection.

Such works as this are calculated to bring about the correct teaching of a *Philosophical Anatomy*.

We extend to this number, therefore, as we have to those which preceded it, a hearty welcome, and commend it to our readers as a faithful exponent of the science of which it professes to treat.

Nothing will be believed now, but that which one can see for himself; and fortunately the laborers in this field are sufficiently numerous to present the science of anatomy in a new light, with no claims but those based simply on facts and their philosophy.

Materia Medica and Therapeutics. By Thomas D. Mitchell, A. M., M. D., Professor of the Theory and Practice of Medicine in the Philadelphia College of Medicine, &c. &c. &c. Philadelphia: Lippincott & Grambo, 1850.

This book is evidently the work of a man of considerable experience, and professes to contain the substance of the author's lectures on Materia Medica and Therapeutics, as delivered in the Medical Department of Transylvania University, in eleven successive winters. As an enlarged and philosophical treatise on Therapeutics, we must say, that it does not come up to our ideas on the subject. The alphabetical arrangement which the author has chosen in preference to a classification of remedies having an analogous Therapeutic application, necessarily breaks the subject too much into fragments, and gives us merely a dry detail of the remedial effects of individual medicinal agents, which, though excellent in themselves, showing great research on the part of the author, and highly useful to the student, does not in fact teach any thing more than would a well arranged Dispensatory.

As a work on Therapeutics, in the extended and scientific sense of the word, we therefore do not approve of Dr. Mitchell's book; as an abridged Dispensatory, with practical Therapeutic suggestions, we do approve of it highly, and think that the research and labor which must have been devoted to the compilation and arrangement of such a mass of material, as honorable to the author

as it is creditable to the profession of which he is a worthy member. The great fault of the book, in our estimation, is, a certain want of dignity and good taste in the general tone of the details; a disposition on the part of the author to raise a monument to his own merits at the expense of the profession; an assumption of superior sagacity and intelligence, the existence of which Dr. M. may in the course of his professional career have often *felt*, but the expression of which, we think, might have been alloyed with advantage by "some few cold drops of modesty." These remarks we make in no ill-natured spirit, but merely to express regret that a man of talent should mar a good work by vanity. For, though the foible is displeasing, we can say of the author as Dr. Johnson said of David Garrick, whom some of his cotemporaries were ridiculing for his conceit: "yes, gentlemen, David is vain, but he has the advantage of most of us in having a great deal to be vain of."

The book contains 730 pages, of as varied material as can well be imagined; therefore we must of necessity content ourselves and our readers with a few quotations illustrative of the author's manner of treating his subject. The Toxicological part of the work is excellent; the methods recommended for the detection of poisons; the preparation and application of antidotes, and the general treatment of patients suffering under their influence, are all good; so much so, that we think that this part of the book alone, whatever its other merits may be, entitle it to high praise, and to a space on the shelf of every medical library.

The alphabetical arrangement, though in our opinion disadvantageous to a strictly Therapeutical work, in which, as we before observed, the massing together of analogous remedies, saves the necessity of frequent and tedious repetition, and gives, we think, a wider and bolder view of the subject, is well adapted to the Materia Medica and Toxicological part of the work, and has enabled the author to give the minute detail of an amount of material which otherwise would have been impossible, without making it too voluminous; which, as we presume the author intended it as a text book for the students of his class, would not have answered his purpose.

The details of Dr. Mitchell's personal experience in the use of remedies are generally good, practical, sensible and reliable, but we object to his constant quotations from Braithwaite's Retro-

spect, Ranking, and the like not that we do not esteem these works as they deserve; on the contrary we think that they sometimes contain much useful information; but they likewise abound in much mere rubbish; new remedies old as the eternal hills, that have been resurrectionized; again buried, and again called into life, to astonish doctors with large organs of wonder and credulity, and to torment their patients.

The fact is, the majority of the contributors to such publications are men of some talent, struggling for a practice; they cannot advertise in the newspapers, because that would be infra dig., and they would by such a proceeding lose caste, therefore the medical journals are the only orthodox means by which they can make themselves known to the public; thus they are constantly on the alert to drag their "soi disant" new modes of treatment before the public, "with twenty mortal murders on their heads to push us from our stools," and which, like Shakespeare's cauldron, manufactured phantoms—

> "Show his eyes and grieve his heart,
> Come like shadows, so depart."

And all this is good and useful in its way; but an over and credulous estimation of such reports is worse than useless, and we contend that it is an over estimation to quote them in standard works, unless proved by the touchstone of experience, as high and reliable authority.

The following quotation is from the author's introduction:

"Possibly some of my readers may be disposed to say that this book should have been called a dispensatory. But having the christening in my own hands, it has seemed good to me to give it the title it bears, and as I think most appropriately. If a dispensatory be not a work on materia medica, why is it made a text book by professors, who teach this department of medical science? The fact is too palpable to be misunderstood by men of sound common sense, that a good dispensatory cannot be a bad exhibit of materia medica; and it is equally manifest that a well digested work on materia medica and therapeutics, even though alphabetically arranged, cannot be a serious hinderance to the study of the teachings of a dispensatory. Vastly fond am I of judicious and real distinctions, but I confess that I have no special regard for a distinction that hardly implies a difference."

We have already given our reasons for considering the alphabetical arrangement as disadvantageous to an enlarged view of

the science of therapeutics, and therefore shall make no further comment.

The subjoined remarks upon the subject of the antagonism of poison and disease, are well and philosophically written. Every medical man of experience must be aware of their truth.

"The point to which I desire to call attention specifically, is one that has entered into my public teachings for many years, and I am free to confess that I do not yet comprehend it fully. I refer to the fact about which there can be no difference of opinion (I mean as to its existence) that decidedly poisonous doses, so far as bulk or weight are concerned, have been frequently swallowed without material injury, and that, too, independently of any condition of the stomach sufficient to account for the result. The doctrine that has appeared to me as the true solution of the problem, is that in the most striking cases on record, the otherwise poisonous dose has spent its force on the morbid action present in the system, whatever that may have been, and in this way its legitimate character has not been developed. I have not met with a single direct reference to this view of the case anywhere, excepting in a short article lately published by Dr. Beck, in which there was an incidental allusion to it. I am aware of the effects of habit in controlling the mischievous action of poisons so as to render them quite harmless; but I have no reference to this agency on the present occasion, for that could not meet the difficulty."

"The case most familiar to the profession, illustrative of this doctrine, is the administration of large doses of tartar emetic in the treatment of pneumonic inflammation. Here the dose is often so great as to be exceedingly hazardous if its operation were restricted to the stomach. The idea of tolerance is associated necessarily with the fact that some other organ besides the stomach is to participate in the agency of the remedy; that other organ is the hinge which feels the influence of the medicine in its restoration to the condition of health. Now although we cannot demonstrate all this just as if it were a problem in Euclid, we are compelled to believe that the salutary influence of great doses of tartar emetic in this disease involves the principles which it is our purpose to illustrate.

We do not desire to be understood here as advocating this plan of curing pneumonia, but simply as attempting to account for the result by the obvious antagonism of disease and poison."

The mammoth doses of nitrate of potassa administered latterly in cases of acute rheumatism, are given by Dr. Mitchell as another illustration of this point; and he might have added the enormous doses of calomel which have been administered in fevers, and in Asiatic cholera, without producing salivation.

In speaking of tobacco as an antidote, or a remedy in poisoning by arsenic, he writes as follows:

"A portion of arsenic fully sufficient to kill under ordinary circumstances, was counteracted by the use of tobacco and without any emetic action. What were the facts with regard to the pathological condition of the stomach? The arsenic had commmenced its appropriate action beyond doubt, as the symptoms evinced: there was very probably set up true inflammation, such as arsenic is competent to establish. How then were the patients saved? The tobacco was given in strong infusion, expressly with the design of vomiting, and so dislodging the poison. But the emetic action failed entirely, and yet the patients were restored after comparatively little suffering.

I know that it has been said that the arsenic and tobacco formed a neutral mixture, in which the poisonous property of both articles was lost or nullified. But it is more probable that the intrinsic character of tobacco failed to display itself, because its power was spent on the existing morbid condition of the stomach and bowels."

The author, in treating of counter irritation with tartar emetic, recommends the following combination:

℞.	Emet. tart.	℈i.
	Oil Olivar.	ʒss.
	Oil Croton.	3ss. Mix.

We must confess that we look upon this prescription as rather heroic. Croton oil, even well diluted, has frequently in our experience, produced very severe effects; emetic tartar, as every body knows, will often produce most painful ulcerations, therefore we do not understand why they should be combined. Dr. Mitchell is evidently a good chemist, and may have discovered, or fancy that he has discovered (which is the same thing now a days,) that croton oil and tartar emetic exert a modifying influence on each other when in combination; but as he has not favored us with any such developement, we ourselves shall continue to use them separately. We must also object, by the way, to the exceedingly unscientific jumble of languages in this formula, for which we know no authority. We are charitable enough to attribute it to oversight on the part of the author, but it is a dangerous example to set before a beginner.

One author has given us a very good article upon the uses and abuses of alcohol, in the course of which he says, and every member of the profession will agree with him, that it is the duty of

doctors to discourage drunkenness: but we protest against the bad taste of the following remarks :

"On account of its active solvent power, (alcohol,) the books abound with tinctures, and there has been too great a willingness to exhibit them. But, inasmuch as a large majority of the sots have been made so by the doctors through the agency of alcoholic medicines, it is the duty of the profession to correct the evil as speedily as the nature of the case will allow."

We have heard this charge made in the lecture-room, and always regarded it as puerile and ridiculous: such excuses may have been made by sots, who think that by accusing the doctors, they excuse their own brutal vices. But what man of common sense will ever believe that a few teaspoonfuls of a medicinal tincture, taken occasionally, ever truly made a drunkard? No; the fellow who could have the impudence to make such a charge, would have been a sot under any circumstances. The thief might just as rationally excuse his career of crime, by stating that having been once, by his wicked father or employer, left alone with a cent, he took it, and could never again refrain. A tendency so strong is the disease itself, not developed, perhaps, but it is there. As Lord Byron says of friendship between the two sexes,

"It is love full fledged, waiting for a fine day to fly."

Cod liver oil seems to be a favorite subject with Dr. Mitchell; for though he does not give us much of his own experience in its use, which we regret, we should have more faith in it than in Dr. Williams' flourish of trumpets, quoted at length by our author, which reads to us very much like the rhapsody of a very well meaning and learned gentleman on the back of a favorite hobby-horse, which he has previously determined to ride nobody knows where.

Cod liver oil, it would appear, is the fashion, and, as was for some time the case with the iodide of potassium, is now prescribed for every thing. Upon the credit of various high authorities it will cure the following diseases:—Consumption, Bronchitis, Pleurisy, Neuralgia, Liver Complaint, Rheumatism, Tertiary Syphilis, Scrofula, and all its incidental ailments, Gout, Dyspepsia, Lepra, Impetigo, Porrigo, &c., &c., &c. So that, in short, the fabulous Elixir of the alchymist is at last realized, and in the hands of the speculating apothecaries, who swept the market of all the stinking oil they could find, has doubtless produced a golden harvest.

We have tried the remedial powers of this oil, and found it of service in some scrofulous affections, but we are sorry to say that we have never yet found it of use in any undoubted case of tubercular phthisis. Still, we are willing to continue the trial, and never let pass a favorable opportunity of doing so; for it is our wish to see this remedy, if it be one for genuine phthisis, take its proper place.

But such a system of puff and exaggeration as has been lately carried on in most of the journals upon this subject, is really as pernicious to the interests of humanity, and the true dignity of medical science, as the worst quackery that ever imposed on the credulity of mankind.

When on the subject of calomel, the author makes the following remarks, which are perhaps judicious, but which we fear might be construed into an apology for ignorance on the part of some blundering or uneducated shop boy.

"As a thing of mere expediency, not to say safety, I hold the word calomel to be preferable, in our written prescriptions, to any other term; and the same remark is of equal force touching corrosive sublimate. By these common names all persons, boys or men, engaged in dispensing medicines, know the one and the other. But all do not comprehend the more correct technicals, which are oftener employed to display the imaginary knowledge of the doctor, than because they are really to be preferred. Fewer blunders would occur, if we invariably used the old terms."

We regret that want of space prevents our quoting more extensively from Dr. Mitchell's book. Notwithstanding the fault which we have presumed to find with certain portions of it, we cannot conclude without repeating our good opinion of it as a whole—recommending it heartily to the perusal of all persons interested in such subjects. To the young practitioner and the medical student we think it will prove a valuable assistant.

The Diagnosis, Pathology, and Treatment of the Diseases of the Chest. By W. W. Gerhard, M. D., Lecturer on Clinical Medicine to the University of Pennsylvania, one of the Physicians to the Pennsylvania Hospital. *Third Edition, Revised and Enlarged.* Philadelphia: Ed. Barrington & Geo. D. Haswell. 1850.

Those who acknowledge the importance of physical diagnosis in diseases of the chest—and who does not?—will hail with pecu-

liar pleasure this new edition of a popular work from the hands of a "master in Israel." The name of its author is closely associated with this subject as one who has done much in its elucidation. This edition contains a variety of new matter, besides a more full development of many portions of the former one. A statement of the effects of cod liver oil in the treatment of consumption is among the novelties introduced. In regard to the question whether cod liver oil ever cures phthisis, the author remarks, that he has "never yet met with a case in which the physical signs, as well as the general symptoms, have entirely disappeared. Indeed, in most cases of the disease, we do not find that the physical signs diminish as decidedly as the general symptoms." The author remarks that sometimes they even increase while the patient is gaining flesh, and his fever is considerably lessened. In these cases the deposition goes on, while the patient becomes less sensible to its effects, and thus even gains flesh. He (the author) therefore concludes that cod liver oil is not a specific against phthisis; it simply increases flesh, notwithstanding the disease; and sometimes may indirectly bring about a permanent cure in cases in which the tuberculous tendency has been ameliorated, and is therefore readily removed. He thinks, too, that cod liver oil will be more certainly useful in those cases in which patients are evidently disposed to consumption, and have inherited a strong tendency to it, although it is not yet developed. Under these circumstances, "where the patient is already thin or slightly emaciated, he directs it to be taken as an article of food rather than as a medicine." In these conclusions we entirely agree, as we have elsewhere in this number, it not yet having fallen to our lot to meet with more than improvement in the general health, in any case of decided phthisis in which we have used it. The following are the results obtained in the Pennsylvania Hospital during a trial of six months; the statement was prepared by Dr. Levick, one of the resident physicians:

"1st. *Of the Oil.*—That the light colored oil can be taken without difficulty by patients whose stomachs have steadily rejected the brown oil.

2d. *Of the mode of administration.*—That a few of the patients have taken the oil without any adjunct to disguise its taste. That its nauseating properties are corrected by its administration with milk; but that its taste is most effectually disguised by the froth of porter.

3d. *Of the time of its administration.*—That as a general rule it has been taken *before* meals, but that in four instances where it was not tolerated before meals it was readily taken after meals.

4th. *Of diarrhœa as a contraindication to its use.*—That the existence of diarrhœa is not a positive contraindication to its use. In three instances in which patients were thus affected, no increase of the symptoms was produced by its use, and no diminution by its abandonment. In a fourth instance, when the diarrhœa had previously existed, the discharge appeared to be increased by the exhibition of the oil, and abated with its withdrawal.

5th. *Of its effects in cases of phthisis pulmonalis.*—That patients using the oil have increased in flesh, in weight, and strength. That while using the oil, their cough and expectoration have diminished; that with some, hectic and rigors have entirely disappeared.

That six of them have been so much benefitted as to leave the hospital and resume their former occupations. That in one instance, a patient who entered the hospital with cough, copious purulent expectoration, extreme emaciation, inability to leave his bed, and with the physical signs of a cavity under the left clavicle, after six months use of the oil left the hospital weighing 140 lbs., with little or no cough, no hectic or rigors, and with an almost entire absence of expectoration: the physical signs having greatly diminished.

6th. *Of the physical signs.*—That the improvement of the physical signs is not coincident with that of the general symptoms.

7th. *Of its use in general scrofula.*—That in scrofulous diseases, where there was no reason to expect the existence of pulmonary tubercles, the improvement of the patient's health has been very decided.

8th. *Of congestion of lungs as produced by cod liver oil.*—That there has been no decided evidence of such a result following the use of the oil in the preceding cases. [Two patients of the twenty while using the oil had severe attacks of hæmoptysis, but there was no reason to refer them to the use of the remedy.]

9th. That in those cases which have terminated fatally, the appetite, the nutrition, and strength of the patient appeared for a time to be decidedly increased; that the life of the patient appeared to be in this manner temporarily protracted; but that for a few weeks immediately preceding death the remedy seemed to have entirely lost its value.

10th. *Length of time, &c.*—That to be of any decided permanent benefit its use must be steadily persisted in. It should be continued even after the most striking symptoms of the disease have in a great measure disappeared."

A short supplement on Spirometry concludes the work, a mode of exploration discovered by Dr. Hutchinson, and of which a brief

notice was given in a review of Kirkes & Paget's Handbook of Physiology in this Journal, and which seems destined to throw light on the subject of physical diagnosis, even in cases where auscultation and percussion fail, as was seen in the case of Freeman, reported by Dr. Hutchinson, in which the Spirometer revealed disease not discoverable by the ordinary means of exploration. The profession owes a debt of gratitude to Dr. Gerhard for the excellent work with which he has presented them, a feeling which we hope will be substantially manifested in the rapid sale of the present edition.

Researches on the Natural History of Death. By Bennet Dowler, M.D., of New Orleans. 8vo. pp. 22.

Various circumstances have interfered with our intention to acknowledge the receipt of Dr. Dowler's characteristic essay at an earlier date. We have further to regret that similar interruptions will compel us to confine the present notice of his views within narrower limits than the importance of the topic may demand.

No one who reflects that in the midst of life we are in death, can fail to take a deep interest in every intelligent and independent discussion of the momentous question to which the doctor's paper is devoted in the present instance. We gladly undertook the examination of these "Researches" therefore, and would still more gladly introduce them to our readers sufficiently *in extenso* to enable them to speak as effectually for themselves to others as they have to us, if such a course were practicable in a brief review of an article which itself is entirely too brief and occasional in character to admit of lengthened critical analysis.

Without attempting "a formal monograph on death," or "even an outline of the several branches of the natural history of death," he has given to his readers, in the course of twenty-two pages, (written, according to his own account, "in fragments, one portion having been in the printer's hands while another remained to be written,") an amount of information and highly suggestive speculation that is not only curious in itself, but entitled to attention as the offering of a physiological observer of established experience and sagacity. Indeed, with all due deference to our

author's other inclinations, or perchance more important occupations, we think his readers would have been much more thankful, and much better able to do justice to his work, had he chosen to spend a little more time and pains upon its elaboration. Grateful as we are for the favor of his lucubrations on the subject, their very richness in fact and illustration has filled us with regret that the formal monograph disclaimed, or, at any rate, that "an outline" were not the actual result. The "preludes" of death, he tells us at the outset, "its characteristics, general and special, its progress, its prognostics, and its pathological anatomy, will be omitted, not for want of facts, but because the facts I have collected on these subjects are too numerous and unwieldy for the narrow limits to which this paper must be restricted. The pathological anatomy of death, the changes due to the agony *itself*, its immediate antecedents and effects, physical and physiological, including the order in which functions cease, and tissues die, together with the resulting anatomical alteration, being subjects of great importance, and deserving of the fullest investigation, cannot be disposed of in a summary manner. The period intervening between the agony and the usual time of post-mortem examination is rich in facts which have been too much neglected." All this is doubtless as true as words can make it, while it tells our trouble at the present writing just as clearly. It is the old story over again—so much to do, and so little time to do it in. Hence our tears. We hear of the great treasure which "still so near us, yet beyond us lies;" we are graciously allowed a glimpse of it, and see and taste just enough of its mysterious wealth to make us hunger and cry for more with all our might.

The leading object of the author seems to be to notice, "more as an experimenter than as a critic," "the criteria of the certainty of death, particularly the tests of death as set forth by the recent proceedings of the Academy of Sciences of Paris, in the award of the Manni prize." He endeavors to show that the means of distinguishing real from apparent death, proposed by the successful aspirant, M. Bouchut, and sustained by the Academy, are not by any means as conclusive as the French savans would have us to believe. But in thus destroying the fair fabric of our Gallic compeers, erected on the discovery of the test by auscultation of

the heart's action, Dr. Dowler hastens to calm the fears of the most timid of his readers with a triple source of consolation.

Not content with assuring us in the strongest manner, what we fully believe, that *bona fide* premature interments are rare almost to an infinite degree, and that the ordinary signs are all-sufficient, he proposes a test of death himself, which he considers more available, as well as more reliable, than that of M. Bouchut. This substitute for auscultation, is not, we are told, the result of mere closet speculation, but "is founded on numerous prolonged experiments, probably one thousand, made directly on several hundred bodies." For the reasons already given in regard to the unavoidable omission of many interesting topics, nothing is said, in this discussion, "of the peculiar physiognomy of the dead, the lividity and cadaveric injection of dependent parts, the flattening of the tissues that sustain the weight of the body, and the other signs of death of minor importance," among which we may enumerate loss of elasticity, opacity of the fingers, the peculiar inward flexure of the thumbs, the expulsion of alimentary substances from the mouth, cessation of capillary circulation, and loss of susceptibility to the action on the skin of external irritants.

Following our author in his own way and order, we have first to call attention to a few remarks respecting contractility and two or three other so-called phenomena of life alone. He repeats, what he has often said elsewhere, that "contractility, animal heat, capillary circulation, and the like, do not prove the body to be alive in the popular, legal, and utilitarian sense." "As to what is commonly called life, the body may be wholly deprived of it without necessarily losing muscular contractility or muscular life." "Life is an aggregation of vital phenomena, or of means and ends; death is a destruction or alteration of these means and ends: all those vital phenomena which remain after the extinction of all the useful means and ends of life do not really constitute life in its ordinary acceptation or essential condition, implying thinking, feeling, willing, and acting." "Contractility, in its isolated character, does not prove that life is present in its ordinary or utilitarian sense; nor is rigidity, as is generally assumed, absolutely incompatible with the contractile power. These forces are neither incompatible nor identical, much less are they connected as cause and effect."

In regard to *rigidity* as a criterion of death, we may as well place in this connection the addendum on that subject with which the tract concludes:

"Rigidity, as a criterion of death, is inconvenient in practice, as it may be tardy in its appearance, and occasionally absent, or of very short duration. Hence its verification requires the constant presence of the physician; otherwise, it might appear and disappear during a short absence from the corpse, throwing doubt on the certainty of the death, and causing delay in the burial. Still, however, it is a sign of great value, and the manner or order in which it disappears is highly characteristic. The suppleness or relaxation of the muscles very generally take place first in those parts that were the first invaded by the rigor mortis, as, for example, in the neck. Rigidity is liable to other objections: it may be simulated; it may originate mesmerically, and convulsively."

Cadaveric rigidity, however, is more easily overcome than the simulated, and when overcome it does not return. It somewhat resembles also the stiffness of congelation, but need not be confounded with it. Rigidity in the corpse is best recognised, as our author intimates, by its successive phenomena in progress, and above all by the peculiar relaxation which succeeds it. Relaxation following rigidity, in the opinion of good authority, is one of the most confirmatory signs.

In continuation of the pamphlet, we next have a very entertaining and instructive historical sketch of the inquiry, especially in its relations to the institution of the prize under the auspices of the Parisian Academy. This, together with a digression by the way respecting early post-mortems in warm climates, and sundry other matters, we would be glad to lay before our readers in the doctor's own words, if the space had been allowed us.

After some five pages of these curiosities of resurrectionary experience, he returns to the Manni prize. This was placed, by a royal decree, at the disposal of the French Academy in 1837, and had been founded "with the hope," for obvious reasons, "of discovering some other satisfactory sign *anterior to that of decomposition.* According to M. Bouchut, the successful candidate, the "certain signs of death are *immediate or remote.* The first consists in 1, the prolonged absence of the sounds of the heart; 2, the

simultaneous relaxation of the sphincters; and 3, the sinking of the globe of the eye, with loss of transparency of the cornea. The first of these, alone, is regarded by the committee as conclusive. The *remote* signs are: 1, cadaveric rigidity; 2, the absence of muscular contractility under the influence of galvanism; and 3, putrefaction."

Dr. Dowler's first objection to the principal or cardiac test, that its application must be limited, "because there are but few good stethoscopists among good practitioners, and in the best hands, certainty is often not attainable," is certainly not a strong one against its scientific value. For granting that the " actual state of auscultation" is so lamentably low, the distinction in this case to be ascertained, is not between certain sounds of the heart under different states of action, but between the presence and absence of any kind of sound whatever—a distinction which can be made by any one possessed of hearing, whether that hearing be previously sharpened by exercise or not. It would be hard to imagine any sound in the thorax, especially in the cardiac region, of a corpse, that could be mistaken for that of the pulsation of the heart ; at all events we are not aware of any yet on record. The next objection is much more to the purpose, although even there the learned experimentalist, in our humble opinion, is running a little too far in his comparative physiology, in spite of the excuse he gives when he cites the hearts of alligators in his illustration of the same organ in man. But let us hear him in his own words:

"The Academy, and M. Bouchut, take for *granted*, that which may not be *true, and which is the very thing to be proved!* Who has proved that the heart, like the pulse, like every other organ, may not fall into *temporary quiescence or inaction?* May not the heart itself suffer apparent not real death, as all analogies drawn from other muscular organs teach? The sphincters, uterus, intestines, and stomach, the respiratory, lingual, ocular and locomotive muscles may be palsied, inactive, apparently dead for a time. It is a downright begging of the question, to assume that the heart *cannot itself fall into this very state of apparent death.* The natural history of the movements of the heart, indicates the probability of a temporary suspension of action; at one time it gives 200, at another 8 or 10 strokes in a minute; it intermits, or is irregular. In cholera it is probably cramped in some cases; temporarily quiescent in others. Moreover, it has, in common with other muscles, a kind of life of its *own;* it is not the known sole criterion of gene-

ral life. Comparative physiology shows that an animal may live hours without the heart, and the heart for days* without the body. An alligator's heart will act with regularity for many hours, perhaps for days, after having been cut out of the body, and emptied of its blood. Let an alligator thus deprived of its heart, be roasted; return its heart, and apply the stethoscope, and then the dead will afford this certain sign of life! The commission of the Academy cannot object to this argument, because they themselves experimented on the inferior animals in testing M. Bouchut's claims: as for myself, I have shown, in my published papers, that I attach less importance to comparative physiology, as the interpreter of human physiology, than systematic writers do themselves. If I take their own point of departure, they can require nothing more.

"M. Dumas, author of the Article *Cœur*, in the Dictionnaire d'Histoire Naturelle, quotes Bacon, Haller, and Diemerbroeck, who state, that in man the heart may be removed without suddenly extinguishing life, and that men have looked about, spoken and prayed after having lost their hearts by the executioner; though M. Dumas does not vouch for the truth of these statements. (iv. 289.)

"Moreover, has any one asserted, much less has any one proved, that the action of the heart is always appreciable; that it never can be so feeble as to escape observation, remote as it is from sight, and even from the hearing? To say with the Academy, that a *prolonged absence* of cardiac sounds, is an absolute proof of death, is vague and unsatisfactory; *prolonged absence* of animal heat, or of respiration, would equally prove the reality of death, not to mention other tests, as rigidity, &c.

The very object contemplated by the Manni prize, is to dispense with this prolongation; for if a prolongation be necessary, there is an infinitely better test; one absolutely certain, the characteristics of which all know as well as the Academy, namely, putrefaction, and which ought not to have entered into the enumeration of M. Bouchut at all. Had M. Bouchut adopted as the criterion of death, the prolonged absence of respiration, the rest had been equally, nay more certain, and, withal, of easier application, than the uncertainties of auscultation. "If the respiration," says Dr. Paris, "be suspended only *five minutes*, we may conclude that life is fled forever. Of all the acts of animal life, this is by far the most essential. Breath and life are properly considered in the Scriptures as convertible terms; and the synonyme, as far as we know, prevails in every language."

*This I cannot vouch for as an observation made by myself, but Dr. Lindsay, of this city, has seen the separated heart of the alligator still in action, on the second day after its removal from the body, which I fully believe, though I have never watched the heart more than 6 to 9 hours continuously after separation.

" As M. Rayer, the reporter, and his coadjutors of the committee, regard this auscultatory test as the principal feature of M. Bouchut's essay, it is proper to look a little further into this matter. M. Rayer and the commissioners made some experiments on the human subject, and on animals; from which they conclude, with M. Bouchut, that " the absence of pulsation of the heart for *five minutes* leaves no doubt of the cessation of life ;" but how many experiments they made does not appear: let us suppose fifty. Now from the very nature of the case, this is but a negation; a *non sequitur;* for it might have happened, that all, or a portion of the next fifty cases would have revived after a *temporary cessation* of the heart's action. Suppose the commission had tried the non-respiratory test for the same period, namely *five minutes*, would they have found any revivals? Would they not have been able to draw a conclusion equally certain ?"

In conclusion of this branch of the argument, we may as well annex another " *addendum*" of the author, which he has inserted as an afterthought at the end of his pamphlet. It is no doubt well known to many of our readers.

" Mr. John Hunter, the celebrated surgeon, long before death, had according to his own statement, (confirmed by his medical attendants, Sir George Baker, and Doctors William Hunter, Huck, Saunders and Fordyce,) an alarming spasmodic attack, in *which the heart's action entirely ceased for three quarters of an hour.* 'This curious fact in physiology, says his biographer, has never been satisfactorily explained.' Mr. Hunter's intellectual powers remained unimpaired. He sustained his respiration by forced, or rather voluntary efforts."

Next we are treated to the " Shaksperian criteria of death." These " will probably not fail once in a thousand years, if ever." We omit the verse, giving only the paraphase in prose: " The circulation ceases; the body cools; the breathing ends; an ashy paleness replaces the natural hues; the cornea grows dim and relaxes; rigidity prevails. These signs may safely challenge, for certainty, all those of the stethoscopes of the whole academy."

The ordinary tests of respiration, such as the motion of the chest, ascertained by sight, and the action of the breath itself in misting the looking glass and agitating the suspended feather, although good tests, may be fortified, according to our author, " by other respiratory phenomena which have been little noticed in this connection, by poets or physiologists, namely—the *peculiar progressive, or rather retrogressive manner by which respiration*

recedes from the lungs to the trachea, from the latter to the larynx, from the larynx to the mouth, and from the mouth to the very lips; this is characteristic, nay, conclusive, if I may judge, of true death, though I do not know that it has ever been regarded at all. It is indeed very different from the cessation of breath in cases of suspended animation, or apparent death, in hysteria, catalepsy, croup, convulsion, stagnation, fainting and the like. The manner of the cessation of respiration, though indescribable to the inexperienced, is very peculiar, and is the earliest absolute sign of real death. The præ-mortem signs are very conclusive." This is all, doubtless, very true, but we need hardly stop to say why their applicability as tests must be limited to a comparatively small number of cases. M. Bouchut's second great sign of death, sustained by the academy, the relaxation of the sphincters, is totally discredited by Dr. Dowler. In denying its authority, the latter speaks from " an experience in this particular line that probably has never been equalled—an experience incidental to several years' experiments on animal temperature—a single experiment often costing several hours, during whieh thermometers have been repeatedly passed within the sphincters." " The latter," continues the Doctor, "with very few exceptions, contract strongly after death. Relaxation of the sphincters is not a post mortem, but an ante or præ-mortem phenomenon. It happens sometimes as a disease, and is not a fatal symptom."

The third and last sign relied on by M. Bouchut and the Academy is not more satisfactory to our investigator. He insists with great reason that it fails in uniformity, whether we study its condition before or after death. We know that it is sometimes a well marked ante-mortem symptom, and that at other times it is absent for some time after vitality has ceased. So much for the Academy and its chosen apostle. We have seen the different fragments of their really noble monument to the philanthropy of the Italian abbé, and their own scientific enterprise, one by one assailed, if not actually demolished. We turn now with renewed interest to the single pillar which our worthy friend has undertaken to plant in opposition to the more imposing structure of his brother savans. Let him speak to us once more in his own language :

"I propose the thermometer as a means of testing death, possessing, as it does, superior certainty over the stethoscope. The latter

method takes for granted, that in apparent death, the heart's action continues; that it cannot be for a time suspended, and that its action can always be heard! The very analogies of apparent or temporary death seem to oppose or contradict these assumptions. The analogies and the positive facts known of animal temperature, teach that, during life, the body is not heated and cooled like inert matter. Place two or three thermometers in the arm-pits—in the bend of the arm, (the fore-arm being flexed,)—in the mouth and within the sphincters, to ascertain the heat of the surface, and of the centres, (the rectum is the best and most accessible centre.) The application of the thermometer requires no skill, and is open to the inspection of all, and is a test for all the warm blooded animals, at least for man. While the auscultatory test takes for granted that there can be no temporary inaction of the heart, and that all its motions can be heard ; the thermometrical test takes nothing for granted without the most indubitable proof. Its great axiom is that man, in his living state, maintains an uniform temperature, independent of the surrounding media, while a dead man, like other inert matter, has no independence of this kind, but steadily responds to, and is governed by, calorific conditions altogether physical—heating and being heated, receiving and radiating caloric. This is not the result of speculation, but of prolonged and varied experimental research.

"The refrigeration of the body before death, in cholera, congestion, and the like, is not physical refrigeration, responding to the calorific condition of the surrounding media ; it is a morbid, or physiological caloricity, which, for a time, augments or continues stationary after death, until it shall be replaced by physical refrigeration, as its phenomenal history clearly shows.

"The facts which I have published concerning post mortem caloricity, do not invalidate this thermometrical test; for soon, or late the physical refrigeration must take place. I may here add, that the speculative opinion which prevails among those who do not take the trouble to make experiments, namely, that these calorific movements are the effects of putrefaction, is wholly unfounded (so far as it regards the human subject ;) how much soever it may be countenanced by certain analogies derived from other inert matter. The calorific, and the putrefactive periods, so far from coinciding, antagonize each other, so long as the heat is not in accordance with the ordinary physical laws of caloric. The point of coincidence and equilibrium, is really the point of putrefaction, unless the circumstances be of an extraordinary character, such as involve the freezing point, or that of torrefaction. But the predomination of the invariable law of physical refrigeration, is a criterion always attainable, and may be proved, as to its times, distances, and velocities, by arithmetical calculation; ascertain the temperatures of the media,

and of the heated body; the velocity of the refrigeration will be proportioned to the times and distances, and will proceed from the surface to the centre, until the equilibrium be attained. The only objection that lies against this rule relates to calorific conditions, where the differences between the heated body and the media are very slight; but this is of no importance in practice, because there is always a marked difference between the average temperature of the air in the shade, and that of a living, or recently dead person."

In thus bringing forward his substitute for the Bouchut test, Dr. Dowler seems to have covered the ground in behalf of its superior certainty and availability, and to have met the practical objections to it well enough to satisfy an attentive reader previously familiar with the subject. It is a matter of regret, however, that in the introduction of so important an adaptation of a striking physiological truth, he has not taken the time and trouble, albeit at the expense of other matter, to explain his views, and especially his practieal deductions, with more fulness and precision. It would not have been hard for him, with his idea of its extreme simplicity, to bring the application of the test he advocates, at least as much within the reach of general readers, as M. Rayer and Bouchut have brought the test which he rejects. The philosophy of vital heat, and of its præ-mortem and post-mortem changes in different parts of the body, under various internal and external influences, including the best mode of appreciating their condition and their results, is not quite so easily comprended, even by the trained observer. It is certainly not less puzzling than the simple axiom of the incompatibility of the slightest sound of cardiac pulsation with actual death, and *vice versa.* There is no doubt about the change of temperature after death; and he who runs may read how low the thermometer has gone in any given case and in the course of a given time. But it is not every one who can so calculate "the times and distances" as to tell how low the thermometer ought to go, or how rapidly it ought to fall.

Dr. Dowler is not singular in his denial of certainty to the inaction, or inappreciable action, of the heart as an evidence of death. The merely negative assertions of the Academy, founded on a limited experience, which determined five minutes to be the longest possible suspension of the cardiac action during life, had been nullified by the positive data of Braschet and others (supposing

them to be worthy of respect) before the scruples of their American antagonist had been presented to the public. M. Braschet informs us in his protest,* that he had repeatedly restored the vitality of infants, in whom no pulsation whatever could be discovered, from fifteen to thirty minutes, although his mode of sub-diaphragmatic exploration (i. e. the application of one or two fingers under the ribs, and between the diaphragm and liver directly under the pericardium,) practised by himself extensively for thirty years, is, in his opinion, as effectual as the nicest auscultation. This writer furnishes his readers with two recent examples, in point. The first was that of a child, who had revived after *twenty minutes'* insufflation, although during that time no trace of pulsation could be heard or felt. The other was that of a man æt. 33, whose heart presented no contraction that could be detected during at least eight minutes, although the ear was applied again and again. *Twenty minutes* after the suspension of its action a slight contraction was perceived in the heart; its pulsations then became regular, and the patient opened his eyes. "Boiling water was thrown upon the limbs" of the poor man, and every possible external stimulant was applied, doubtless including artificial heat; so that, in the latitude of New Orleans, he might have been stripped for the tender mercies of the ice-box or dissecting table, or perchance enclosed in his coffin for immediate interment, almost before refrigeration could have had a chance to display its progress on the most susceptible thermometer. A medical friend of ours, in the course of a steamboat voyage up the Mississippi last summer, was present at the digging of a grave on shore, intended for a fellow-passenger, who was dying of cholera. The poor creature had gone on with his apparent refrigeration, while his provident undertakers were at work upon his contemplated lodging. He disappointed them at last, however; for when they were ready he was not, inasmuch as, although the heat had left his body, the breath was still in it. We do not know that such *contre-temps* as this ever happen in New Orleans to embarrass the preparation for the premature examinations which are common in that city. Sure we are, however, that we should have little faith in any sliding scale of refrigeration, however central in its appli-

* Gazette des Hopitaux, No. 135.

cation, while such a complexity of horrors were menacing our closing hours. With the scalpel already bared on the one hand, a yawning grave on the other, and no other admissible alternatives but the ice-box or putrefaction, we would rather a thousand times encounter the worst exhalations of a dead-house than trust the disposal of a friend's remains to the decision of the best thermometer that ever was made.

After all, the difficulty is vastly more apparent than real. At the present day, especially, is it infinitely more serious in theory than in practice. Premature interments and marvellous revivals appear frequently enough among the items of the newspapers; but in the grave-yards themselves, at all events in the British, German, and American burying-grounds, such accidents are utterly unknown. It is otherwise among the French, if we are to listen to the startling representations of their numerous writers on the subject. There is every reason, however, to accord with Dr. Dowler and the London Quarterly Review* in estimating the statistics and various romantic stories of Bruhier, Louis, Thierry, Nysten, Fontenelle, and others, as so many draughts on the credulity of a wonder-loving public.

The signs of death, properly so called, should be studied under different aspects. First, when the opportunity allows, they should be watched in their progress before, as well as directly after, the final change; secondly, in their succession after death; and, lastly, in their assemblage as a whole. Many of them are important individually, but it is in the last named general view that they become, in spite of adverse theory, sufficiently conclusive for all practical purposes.

Having disposed to his satisfaction of the prominent points in the discussion of the criteria of certain death, our author continues his "Researches" in some seven pages of highly interesting miscellaneous matter, embodying a variety of curiosities of medical and philosophical experience, chiefly relating to the "*agony*" of death. This portion of the pamphlet we are compelled to pass without further comment than to recommend it to the attention of all who have time to avail themselves of the inexhaustible ingenuity and reading of its author.

To return for a moment to the question of the difference be-

* No. clxx, Oct., 1839.

tween real and apparent death, we cannot do better than to conclude with the closing survey of a recent French reviewer in the *Gazette Medicale.* We present it as much for the summary it gives as for the precept it enjoins. "Experience," says he, "has shown the insufficiency of each of these signs, with one exception—*putrefaction.* The absence of respiration and circulation, the absence of contractility and sensibility, general loss of heat, the hippocratic face, the cold sweat spreading over the body, cadaveric discoloration, relaxation of the sphincters, loss of elasticity, the flattening of the soft parts on which the body rests, the softness and flaccidity of the eyes, [the loss of catoptric reflection too, and opacity and dryness of the cornea,] the opacity of the fingers, [and, we may add, the peculiar inward bending of the thumbs,] cadaveric rigidity, the expulsion of alimentary substances from the mouth; all these signs combined or isolated, may present themselves in an individual suffering only from apparent death. Doubtless their complete co-existence, except in case of actual death, is extremely rare. Doubtless, too, some of these signs have so discouraging a signification, even separately, that their presence is equivalent to a sentence of death. Still, in so grave a matter the very rarest exception possesses an infinite value. We will go farther and say, that it ought even to weigh as much as the rule itself in governing the conduct of the physician and attendants. Here, if ever, is the axiom, *Melius anceps quam nullum,* to be applied. Were the chances of success only one in a thousand, one in ten thousand, or a hundred thousand, every precaution, every measure likely in a remote degree to realize such success, becomes a bounden duty."

Human Physiology. By Robley Dunglison, M. D., Professor of the Institutes of Medicine in Jefferson Medical College, Philadelphia, Vice President of the Sydenham Society, London, &c., &c. *With nearly five hundred illustrations. Seventh Edition. Thoroughly revised, and extensively modified and enlarged.* In two vols. Philadelphia: Lea & Blanchard, 1850.

In our last number we announced a fourth edition of the well-known work on Materia Medica and Therapeutics by the author of the above treatise, and close upon it follows a seventh edition of the "Human Physiology," thoroughly revised and modified—a labor which those only who have kept pace with the rapid strides of Physiology, and the kindred sciences of General Anatomy and

Organic Chemistry, can appreciate. The author informs us that the whole work has been subjected to a rigid scrutiny, both as to the doctrines contained in previous issues, and the language in which they were conveyed, and the result is a work of all the evenness of style so much to be desired in the production of a treatise to be placed in the hands of the younger portion of scientific inquirers. Some idea of the labor expended upon this edition may be had from noting, that the bibliographical list of authors referred to in its preparation extends over nine closely-printed pages, and includes all that has appeared, either in distinct treatises or monographs, since the last edition. As to the manner in which the work is executed, those who are familiar with previous editions need but to be told that this is fully brought up to the day, and presents faithfully the existing state of the science. Where all is excellent it is hard to particularize; we therefore commend the book to the student as the best exponent of the science of Physiology with which we are acquainted, and can heartily sympathize with Dr. Dunglison in the satisfaction with which he has produced a work which redounds so much to his credit, and to that of the profession of which he is so valued a member. The typographical execution is excellent. We notice that many of the old cuts have been replaced by new ones, while additional ones have been introduced, thus greatly enhancing the value of the work.

A Practical Handbook of Medical Chemistry. By John E. Bowman, Fellow of the Chemical Society, Demonstrator of Chemistry in King's College, London, and Author of "Practical Chemistry." Philadelphia: Lea & Blanchard, 1850.

The little work whose title heads this notice fills the vacancy that seemed to be left in the "Introduction to Practical Chemistry" by the same author, which was brought to the notice of our readers about a year ago. To the student of medicine and the practitioner this will prove particularly acceptable, by supplying him, in a clear and compendious form, with information too rarely obtained from his teachers, especially in relation to the analysis of urine, blood and the detection of poisons in organic mixtures, and which most frequently he obtains only through the assistance of others, who have more leisure or more experience than himself. The work is divided into five parts: Part I. is of Urine. The first Chapter

contains an account of healthy urine, and its various constituents, Chapter 2. The Qualitative Analysis of Healthy urine. Chapter 3. Average composition of Healthy urine. Chapter 4. Morbid urine, and the various extraneous matters met with in it. Chapter 5, is on the Qualitative examination of urine suspected to contain either an unnatural proportion of some one or more of the usual ingredients, or some abnormal matter. The various abnormal substances are detailed, and plain directions given for their detection. Chapter 6, is on the Examination of Morbid urine, the nature of which is unknown. Chapters 7 and 8, relate to the qualitative analysis of diabetic and albuminous urine. Part II. relates to Calculi and their concretions, both urinary and biliary. Part III. is on the blood—its quantitative and qualitative analysis. Part IV. describes Milk, Mucus Pus, Bone, etc.; and Part V. and last treats of the detection of poisons in organic mixtures, etc.' of the former of which the most common are detailed, in connection with their tests; to which is added the method of examining an organic mixture suspected to contain some mineral poison, the nature of which is unknown. From this list of the contents of the work, our readers will be enabled to form an estimate of the amount of useful matter contained in it. We commend it most cordially to their sedulous examination as a reliable handbook, more especially to those whose locations do not admit of ready appeal to others more conversant with these important aids to diagnosis.

British and Foreign Medico-Chirurgical Review. July, 1850. *American Edition.* New York: R. & G. S. Woods.

We have received the American reprint of this valuable periodical, which is offered to the profession in this country at the moderate rate of three dollars per annum.

THE MEDICAL EXAMINER.

PHILADELPHIA, OCTOBER, 1850.

COMMENCEMENT OF THE WINTER SESSIONS.

Before another number of our Journal shall be issued, our various Medical Schools will be actively engaged in the responsible duty of professional education. Already do we notice the appearance in our thoroughfares of those who have sought Philadelphia for her well known and justly appreciated advantages in medical instruction. Great as may be their anticipations of a rich harvest, we feel assured that they will not be disappointed, but that they will return to their homes at the close of the session more deeply than ever impressed with the conviction that here may be attained all that the most zealous and aspiring student can desire. It is predicted by those knowing in these matters that the classes at the various schools will be larger than at any previous season. We trust that their anticipations may be realized. It is fortunate that our extended territory will give the young aspirants room to flourish in after graduation.

Drs. S. D. Gross and Elisha Bartlett have been appointed to the chairs of Surgery, and Institutes, and Practice of Medicine in the University of New York. Both these gentlemen held the same chairs in the University of Louisville, of which institution they were valued and efficient members. Dr. Gross, is well known as the author of an admirable and standard work on *Pathological Anatomy*, and as editor of *Liston's Elements of Surgery*. And Dr. Bartlett as author of a work *on Fevers*, most favorably received by the profession; and of an *Essay on the Philosophy of Medical Science*, and one on *Certainty in Medicine*.

The faculty of New York University have added to their strength in these appointments, and we congratulate them on having secured the services of such experienced collaborators.

Dr. Baxley of Baltimore has received and accepted the appointment of Professor of Anatomy in the Medical College of Ohio, to fill the vacancy created by the death of Dr. Shotwell. All the chairs in this Institution are now full, presenting an able and accomplished corps of teachers.

DEMONSTRATIVE MIDWIFERY, AGAIN.

We stated in our last number, that the favorable opinion of this method of teaching, as once expressed by us, and since withdrawn, was based upon *erroneous information*. We wish it to be distinctly understood that that information was not derived from *any one connected with Buffalo Medical College, either directly or indirectly.*

QUERIES ON THE INFLUENCE OF TOBACCO.

We have been requested by a much respected friend, to lay before our readers, some queries recently addressed to him by an English Surgeon in relation to the influence of Tobacco upon the health, in hopes that some of our readers would be able to afford the desired information. He says, " many circumstances of late have occurred, in which I have seen the most injurious effects of the use of tobacco upon the nervous, circulatory, and digestive functions. A friend of mine became a perfect hypochondriac by the use of snuff, and was at once relieved by leaving it off. Upon returning to the use of it, he again suffered as before, and was again relieved by ceasing to take it. Here there was no doubt. Another gentleman was covered with an eruption resembling psoriasis, from head to foot, and got well immediately when he left off the use of snuff. Three times he suffered a relapse upon taking snuff, and was cured by leaving it off. In many smokers, I may say all, I have found heart disease or confirmed dyspepsia. If you can help me with any statistic accounts of disease of the heart and arteries, of brain and nervous system, and of the stomach and chylopoietic viscera, and cancer of the mouth and lips, I should feel greatly obliged. If to these you could add any data of the use of tobacco by the sufferers, it would greatly enhance their value."

The subject is a deeply interesting one, and we trust that out of the large experience of our readers, something may be gained to illustrate the effects of this agent upon the economy.

DEATH OF DR. HARTSHORNE.

Died at *Brandywine Springs*, near Philadelphia, on the 20th of August, Dr. Joseph Hartshorne, aged 71 years.

Dr. H. was one of the oldest practising physicians in Philadelphia, and until within a short period of his death was actively engaged in professional duties. The virtues of his character, and the high estimation in which he was held by his associates, are better expressed by the subjoined resolutions, than by any words of ours; to them, therefore, we eave the grateful duty of commemorating them.

At a session of the College of Physicians of Philadelphia, specially convened on the 26th of August, 1850, that the Fellows might have an

opportunity to take such action in reference to the loss they had sustained in the demise of their late colleague, Dr. JOSEPH HARTSHORNE, as their feelings should dictate: the Vice President, Dr. Charles D. Meigs, introduced the subject of the meeting by a few remarks on the professional standing and private worth of the deceased; after which, the following Resolutions were presented by a Committee, consisting of Drs. Bell, Parrish, and Hallowell, appointed to prepare them, and unanimously adopted.

Resolved, That the College learn with deep emotion, the melancholy announcement of the death of its esteemed Fellow, Dr. JOSEPH HARTSHORNE.

Resolved, That in giving utterance to their feelings at the event, the Fellows of the College are sure to express, at the same time, those of the Profession of which the deceased was so long an eminent and esteemed member, and of the community amid which he toiled so faithfully and so ably.

Resolved, That the example given by the deceased, of devotion to his professional duties; of great skill joined to a frank and manly bearing in the exercise of them, be received by us as an example worthy our imitation.

Resolved, That a Fellow of the College be appointed to prepare a biographical notice of the deceased, to be read before the College, and inserted in its Transactions.

Resolved, That a Committee be appointed to communicate to the family of Dr. HARTSHORNE, the sympathy of the College in their bereavement.

To the gentlemen composing the Committee by whom the foregoing were prepared, was delegated the duty imposed by the last resolution.

It was further resolved, that the proceedings of the meeting be published in the Medical Journals of this city.

D. FRANCIS CONDIE, Sec'y.

MILITARY SURGEONS IN FRANCE.—SPEECH OF M. DUPIN, PRESIDENT OF THE NATIONAL ASSEMBLY.

I have the honor of addressing the military surgeons of the French army, and I tell you, if any one were bold enough to still dispute your right to proportionate rank in the army, you might proudly answer by pointing to this statue, and by citing the life of the illustrious man whom it represents—the life of the worthy Larrey.

I have said it elsewhere, and I will repeat it whenever an opportunity offers—the military surgeon, fearless in epidemics, fearless in the field of battle, possesses all kinds of courage. He has military courage, because he faces death, offered on all sides by fire and sword; and another courage, far superior to this, for he preserves his calm coolness and presence of mind when his life is in the greatest peril. The blow which is aimed at him, and which he sees threatening, cannot, and even were he able, would not, be returned by him. He knows his hazardous situation, and does not hesitate to fulfil his dangerous duties.

Kneeling by the side of the disabled, with as firm a hand as when he is studying nature in the anatomical rooms, he dresses their wounds. But with these two kinds of courage he reaps two kinds of glory; and Larrey, who has shown his courage equally in both, now deserves to be honored with double glory. He has proved, when twice wounded. that the dangers which the military surgeon runs are not imaginary He was wounded once in Egypt, in times of glorious memory, and another time at Waterloo, on that mournful day for France.

You heard, from those who addressed you before me, what the life of Larrey has been, and what services he has rendered to science. It is not for me to enter into the details of his noble career; I am, besides, not prepared for it; I speak, carried away by momentary impulse, and by the admirable speeches which have just been uttered. I judge this learned man—this defender of mankind—by considering, as a whole, his useful life, marked by the most enlightened and noble services, and I bow before this statue which so worthily represents him. Yes, I greet Larrey! the virtuous, devoted man; whose self-denial and devotion triumphed even over the elements, and who has been among us as an incarnation of genius and humane feelings. He deserves the thanks of science, the army, France, and the whole civilized world.

Dublin Med. Press.

ARMY ORDER—RELATIVE RANK OF ARMY AND NAVY OFFICERS.

Washington, Sept. 26th, 1850.

The following Army Order was issued by the War Department on the 21st inst.:

"The House of Representatives having on the 18th of July, 1850, adopted a resolution, requesting the President to communicate to that Honorable body his views of the rules and regulations which should be established by law on certain subjects therein mentioned relating to rank in the army and navy, the President directs that a board of officers of the army be assembled, who shall deliberate on so much of said resolution as appertains to the army, and shall consult with any similar board, composed of officers of the navy, that may be appointed for the purpose, in relation to so much of said resolution as relates to the relative rank of the army and navy."

The following officers of the army are appointed members of the board: Gen. Scott, president; Gen. Jessup, Gen. Wool, Col. Crane, Col. Waite, Surgeon Mower, Paymaster Hunter, Lt. Col. Scott, recorder. The board will assemble at Washington, on Monday, the 14th of October, and after closing their proceedings, will report to the War Department their views and opinions on the subjects submitted to them.

We hope that the subject of *assimilated rank* will be brought before this board, and that our medical brethren in both services will at last have justice done them.

Deaths in Philadelphia from Aug. 24th to Sept. 21st, 1850. Reported by Mr. JAMES AITKEN MEIGS, Student of Medicine.

Diseases.	Ad'ts	Chil.	Diseases.	Ad'ts	Chil.
Abscess,	0	1	Fever, typhoid,	11	3
Anæmia,	0	1	" typhus,	6	0
Angina pectoris,	1	0	Gangrene,	1	1
Apoplexy,	6	0	Hæmoptysis,	2	1
Asthma,	2	0	Hemorrhage	1	1
Burns,	1	2	" uterine,	3	0
Cachexia,	0	1	Ileus,	1	0
Cancer,	2	0	Inflammation of brain,	5	16
" breast,	1	0	" bronchi,	0	10
" stomach,	2	0	" liver,	2	1
" uterus,	2	0	" lungs,	5	14
Cancrum oris,	0	2	" ovaries,	1	0
Caries of jaw	0	1	" peritoneum,	5	2
Casualties,	1	1	" pleura,	0	2
Cholera infantum,	0	48	" stom. & bowels,	10	11
Compression of brain,	0	1	" throat,	0	1
Concussion "	0	1	Inanition,	0	5
Congestion of lungs,	0	2	Intemperance,	6	0
" brain,	2	6	Ischuria,	1	0
Convulsions,	2	27	Jaundice,	1	0
Croup,	0	7	Kidneys and liver, fatty degeneration of,	1	0
Cyanosis,	0	2	Malformation,	0	1
Debility, general,	5	11	" of heart,	0	1
Diabetes,	2	0	Malignant disease of bones,	0	1
Diarrhœa,	8	16	" sore throat,	0	1
Disease of brain,	0	8	Mania-a-potu,	3	0
" heart,	5	1	Marasmus,	0	34
" kidneys,	1	1	Measles,	0	2
" liver,	2	0	Mortification of hand,	1	0
" lungs,	2	1	Obstruction of bowels,	1	0
" ovaries,	1	0	Old age,	22	0
" stomach and bowels,	0	2	Ossificat. of valves of heart,	1	0
Dropsy,	6	4	Palsy,	6	0
" abdominal,	1	0	Pertussis,	0	21
" of breast,	1	0	Phthisis pulmonalis,	47	13
" head,	0	26	Poisoning,	2	0
Drowned,	7	7	Purpura hemorrhagica,	0	2
Dysentery,	33	41	Rupture of uterus	1	0
Effusion on brain,	2	7	Scrofula,	1	2
Epilepsy,	2	0	Small pox,	0	2
Erysipelas,	1	0	Softening of brain,	0	1
Fever,	1	3	Spina bifida,	0	2
" cerebral,	0	1	Still born,	0	40
" congestive,	1	0	Suffocation,	2	1
" hectic,	0	1	Syncope,	1	0
" intermittent,	1	3	Syphilis,	1	0
" puerperal,	1	0	Tabes mesenterica,	1	5
" remittent,	6	2	Teething,	0	8
" scarlet,	0	12			

Diseases.	Ad'ts	Chil.	Diseases.	Ad'ts	Chil.
Tetanus, . . .	0	1	Unknown, . . .	7	2
Ulceration of small intest.	2	1	Violence, . . .	4	0
" colon, .	1	0	Wound, . . .	2	0
				269	459

Total, ... 728

Of the foregoing the ages were as follows:—

Under	1 year,				207
From	1	to	2,		116
	2	to	5,		82
	5	to	10,		26
	10	to	15,		13
	15	to	20,		15
	20	to	30,		64
	30	to	40,		61
	40	to	50,		40
	50	to	60,		32
	60	to	70,		31
	70	to	80,		21
	80	to	90,		17
	90	to	100,		3
					728

Included in this number, are 53 from the Almshouse, 14 from the surrounding country, and 58 people of color.

RECORD OF MEDICAL SCIENCE.

ANATOMY AND PHYSIOLOGY.

Comparative size and shape of the Thyroid Foramen in the Male and Female Innominatum. Dr. NEILL called the attention of the Fellows of the College to the comparative size and shape of the Thyroid Foramen in the Male and Female Innominatum.

He believed that many teachers of Anatomy and of Obstetrics were in error upon this subject, while others had failed to point out the difference between the male and female pelvis in this respect; that this was the case especially in this city, and perhaps in this country generally.

He had learned that Dr. Wistar and Dr. James taught that in the *female* the foramen was *oval*, and that in the *male* it was *triangular*, although there was no statement upon the subject in the old edition of Wistar's Anatomy which he had examined, nor in the more recent edition known as Pancoast's Wistar. Dr. Horner also stated in his

work, that "in the male it is triangular, in the female, rather oval." On the other hand, Meckel, Cloquet, Cruveilhier, Harrison and Quain and Sharpey make a statement precisely the reverse of this.

The lecturers upon obstetrics with whom he had conversed, either teach the former view, or are silent upon the subject. Denman, Baudeloque, and Maygrier say nothing. Neither do Monroe or Cheselden, both high authority on the bones, nor Winslow, Bell, Bartholin, &c.

Sœmmerring says "the foramen is elliptical in children, and triangular in adults." Wilson and Von Behr say it is triangular in women.

In order to satisfy his own mind on the subject, Dr. Neill had, up to this period, examined thirty-two skeletons, and the result was so contrary to the view which he, and perhaps most of the Fellows had been taught, that he had thought it worth while to prepare a chart, exhibiting diagrams of the male innominata in one column, and those of the female in another, to show at a glance, the distinctive difference.

He believed from an inspection of this, every one would be convinced that the foramen in the *male* is *oval*, while in the *female* it is *triangular*.

It will also be observed that the male foramen is longer and narrower, and that the line representing the long axis is more vertical, and nearly parallel to the rami of the pubes and ischium; whereas, in the female the foramen is not only smaller and triangular, but that the apex of the triangle is downward, that its internal side is nearly parallel to the rami of the ischium and pubes; and that the base of the triangle is proportional to the chord of the arch of the pubes.

He remarked that the establishment of this fact, by investigation or by authority, would not interest the Fellows of the College in a practical point of view, its only value consisting in its affording another mark of distinction between the male and female skeletons.

Trans. Col. Phys. Phil.

PATHOLOGY AND PRACTICE OF MEDICINE.

Case of Poisoning with Arsenic. Death in thirty-six hours—Influence of Arsenic on Digestion—Remission of Symptoms. By A. G. Greaves, Surgeon to the Derby Dispensary.—On the 26th of February, 1847, Mrs. Ann W——, aged 24, in the ninth month of pregnancy, was taken ill at half past 9 A. M., with sickness and vomiting, having an hour previously partaken of a breakfast of tea and bread and butter. The vomiting continued at intervals during the day, the matter thrown up consisting principally of bile. No medical assistance was called in, deceased saying that she thought her illness was owing to her pregnancy.

27th.—Mrs. W. vomited once at 9 A. M., and then felt very much better. She cleaned her house thoroughly during the morning, took some gruel at half-past 12, and at 1 P. M. left home, as she said for a walk. She returned at 2 P. M. and stated that she had been obliged to call at the house of a friend about two hundred yards from her home, having again been sick. At half-past two she again threw up a quantity of what, from the description given, I supposed to be bile. At half-past 4 I was called in, and found her seriously ill, complaining of vio-

lent pain in the stomach and abdomen, with diarrhœa and tenesmus, and great difficulty of breathing. She sank rapidly, and died at a quarter past 7 P. M.

I ascertained that deceased had felt the movement of the child a very short time before her death, and I therefore urged the friends to allow me at once to open the uterus and remove the fœtus; but they objected.

Post-mortem 22 *hours after death.*

Head.—Brain and the membranes perfectly healthy.

Chest.—Lungs contained a considerable quantity of dark blood. The heart was very flaccid, and both ventricles were filled with fluid blood of a very dark character.

Stomach contained about a pint and a half of bloody fluid, on pouring off which I found a very large quantity of exceedingly viscid albuminous secretion containing numerous patches of a white powder, which analysis proved to be arsenic. The mucous membrane was much inflamed.

The duodenum was also inflamed, and contained one or two patches of the same white powder. In the caput coli was found a considerable quantity of *undigested* soup, containing several patches of undissolved arsenic, precisely like those in the stomach.

The bladder was empty and healthy. The uterus contained a fine healthy child, but there was no evidence of labor being near. The membranes were entire, and everything in a normal state. A careful analysis by myself and my friend, Mr. Bernays, shows most unequivocally the presence of a large portion of arsenious acid in the contents of the stomach, but by whom administered there was no evidence to show, beyond a remark of the deceased, "that it would be a shocking thing if she and her husband should be found dead in bed." This led to a suspicion that it was an act of suicide.

We have unfortunately, no satisfactory data to form a conclusion as to when the *fatal* dose of arsenic was taken. There can be no doubt that some of the poison had been administered on the 26th, thirty-six hours before death, but whether that was the dose of which she died is uncertain. It is interesting to remark how large a portion of arsenic remained in the stomach, notwithstanding the continued vomitings the deceased had for five hours before death.

There are some circumstances connected with this case which are of interest in a pathological view. It appears that the deceased partook of of a quantity of soup about *seven* hours before her death; and yet, although this long period had elapsed, it was found in the caput coli in an undigested state, and mixed with a quantity of arsenic. But for this fact being well known, it might have been inferred that deceased had taken a large dose of arsenic in soup three or four hours before death. This appears to show that the action of arsenic materially interferes with the process of healthy digestion.

Another circumstance worthy of notice is, that there was so complete a remission of the symptoms after the poison had been swallowed, that the deceased was able to occupy herself in her usual avocations, and to go for a walk.—*Medical Gazette.*

Treatment of Singultus by Sulphuric Acid. By Dr. SCHNEIDER.—During a long practice, Dr. Schneider has met with many cases of hiccough, occurring in both sexes and at different ages, and persisting with such obstinacy as to give rise to great suffering and exhaustion. His sovereign remedy in such cases is one of the preparations of dilute sulphuric acid, which act with great promptitude. He refers also tc the testimony of Dr. Duncan, in favor of this substance contained in the 'Edinb. Med. Comment.,' and to that of Dr. Jacobsen, of Berlin.—('Casper's Wochenschrift.')—*Brit. and For. Review, Jan.* 1850, *p.* 274.

On the Treatment of Hematemesis and Melœna. By Dr. J. M. NELIGAN, Physician to Jervis Street Hospital, Dublin.—[Dr. Neligan remarks that the greater danger of hematemesis and the greater difficulty of controlling it, as compared with melœna, seem to depend on the size of the stomach permitting a large accumulation of blood in its cavity before it contracts; its contraction "being the natural mechanical method by which the bleeding is checked." Thus, he observes:]

In the administration of remedies to stop the hemorrhage, the chief indication to be fulfilled is therefore manifestly to produce contraction of the stomach, whether the bleeding be symptomatic of organic disease or not. With this view we should be guided in the choice of styptics or astringents, and they should be administered in a concentrated form, or rather as little diluted as possible, so as not to distend the stomach. In hematemesis or melœna, I place most reliance on the oil of turpentine as a styptic, and on gallic acid as an astringent; the former, by its stimulant property, excites the stomach and intestines to contract, and the latter may be given in the solid form, and is very powerful in small doses. Ice has often been prescribed to check hemorrhage from the stomach; but its tendency, when dissolved, is to distend the organ, and to favor further bleeding by diluting the blood that may have previously escaped from the vessels, constitutes, I think, a very serious objection to its employment. As to blood-letting, it is only admissible in those few cases where the hemorrhage seems to be dependent on general plethora, and even in these must be regarded rather in the light of after-treatment than of an immediate remedy.

The diet in all cases of hemorrhage from the stomach or intestines must be absolute. Nourishment should be given in a concentrated liquid form, perfectly cold, and in the smallest quantities at a time; and a return to the usual articles of diet should be permitted with the utmost caution.

I regard gallic acid as our most valuable astringent in hemorrhage from the mucous membrane of the digestive organs, from the uterus—in many cases of bleeding from which I have seen it prove of the greatest service, and from the urinary organs.—*Dub. Jour., May* 1850, *p.* 348.

On the Use of Ergotine in External and Internal Hemorrhages. By M. J. Bonjean, Chambery.—Ergotine when applied to wounds has the property, M. Bonjean states, of facilitating their cicatrization and moderating inflammation of the wounded tissues. Under its influence union takes place by the first intention, and cicatrization occurs without further assistance.

In certain cases ergotine may perform all the offices of the ligature. M. Bonjean enumerates the following circumstances attendant on a capital operation in which its employment was indicated:

1. When, in order to arrest a hemorrhage, it would be necessary to disturb the lips of a wound in which cicatrization is commencing.
2. When the patient manifests a tendency to gangrene of the cut surfaces.
3. When the source of the hemorrhage is from vessels imbedded in the inflamed and swollen tissues.
4. When the blood flows from many small arteries of which the orifices cannot be perceived.
5. When hemorrhage occurs from the sloughing of an eschar, as in gun-shot wounds, &c.

In these difficulties the application of ergotine is as often efficacious as the use of pressure is ineffectual. The application of ergotine supersedes ligature of the arteries, and effects cicatrization without interfering with the permeability of the artery.

The mode of employing ergotine is to dissolve it in five or six times its weight of water, for ordinary wounds; and in three or four parts, or even in a concentrated form, for more serious hemorrhages. A portion of the tow or lint is to be moistened with the fluid, and applied with gentle pressure to the surface previously wiped. When the hemorrhage does not return on the pressure being removed, another pledget moistened with the solution is to be laid over the former, and the limb bandaged as usual. Perfect rest is to be observed.

Internal administration.—Ergot of rye has been successfully employed:

1. As an excitant of uterine contractions.
2. As a stimulant to the muscular system in general.
3. In hemorrhages and certain fluxes.
4. In congestion of the uterus.
5. As a stimulant to the nervous system.

The latter poisonous effect of ergot of rye is due, according to M. Bonjean, entirely to its fixed oil. The preceding properties are due to the *ergotine* alone.

Simple extract, or ethereal tincture of ergot, both contain a portion of its poisonous principle. Pure ergotine is in the form of a solid extract of a deep brown color. In thin laminæ it presents a blood-red color. It has the odor of roast meat. Its taste is bitter. It is perfectly soluble in water, and this solution yields neither oil nor resin when heated with ether. (Gazette Méd.)—*Med. Gaz., May* 1850, *p.* 787.

SURGERY.

On "Ingrowing" of the Toe-Nail. By H. J. M'Dougal, Esq.—[Mr. M'Dougal relates the following case: a gentleman was brought to him with an affection of the great toe which rendered him quite unable to walk, from the pain which pressure of the foot upon the ground occasioned. Mr. M'D. says:]

On examination, the toe was found slightly swollen, and with a reddish, erythematous blush extending up the foot. There was a very little fungoid granulation by the side of the nail, touching which was by no means so painful as pressure either on the under part of the toe, or on the upper and inner surface of the nail. The edge of the nail was quite invisible.

I directly proposed the usual operation of division of the nail in the centre, and eversion of the affected side. This had been proposed by two surgeons, whom the patient had previously visited, and was decidedly objected to. Being left to my own resources, therefore, I proceeded to scrape away, with an angle of glass, the inner surface of the nail (holding aside the flesh with the left hand) until its structure had become so thin that, with a pair of scissors, I was easily enabled to divide it for a short distance, and with forceps to lift out the piece in the corner. This gave little or no relief, and I was induced to seek further for the cause of the pain and distress felt on touching the toe. A horny-looking surface filled the space from which the piece of nail had been removed; and, on scraping round this with the point of the scissors, I succeeded in turning out a hardened mass of collected epithelium scales, nearly as large as the seed of a sweet pea. The surface underneath was red, and secreted a sanious matter. Perfect relief ensued on the removal of this extraneous matter, and the patient congratulated himself on his own obstinacy in not consenting to the very painful operation of losing half his nail. A morsel of dry lint completed his cure in twenty-four hours, and a little occasional attention to the part has since saved him from further suffering.

I am not aware that the condition I have described above has been noticed by any surgical writer in our language, with the exception of Mr. Colles, of Dublin, who refers to it as only occurring on one side of the nail. I can quite conceive, however, that with a little attention many persons might be saved the exquisitely painful and barbarous operation now so often used, of tearing asunder the inflamed matrix, confessedly one of the most tender parts in the whole structure of man.—*Med. Times, March 16th,* 1850, *p.* 195.

MATERIA MEDICA AND THERAPEUTICS.

Practical Remarks upon Ipecacuanha, with a formula for a more uniform and efficient preparation of the syrup, than that laid down in the Dispensatories. By Edward Jenner Coxe, M. D., New Orleans.—Before noticing the main object of these remarks, it may prove

neither uninteresting nor unprofitable to direct attention to some of those diseases in which this medicine, or some of its preparations and combinations, may be employed. The value and efficacy of ipecacuanha, as an emetic or expectorant in many affections of the respiratory organs, more particularly of children, are too generally conceded and acted upon to require an extended notice.

In dysentery, ipecacuanha has been and continues to be much used.

By Mosely who held it in high repute, ipecacuanha was given in doses of half a drachm to two scruples, and by the late Professor B. S. Barton, it was regarded as almost a specific, particularly in cases of a typhoid character. In chronic diarrhœa, small doses of the powder repeated several times a day, either alone, or preferably in conjunction with opium or Dovers powder, will be found of great value and frequently, with strict attention to a proper regimen, will succeed in curing many most unpromising cases.

In these last cases, when dependent upon, or connected with derangement of the biliary secretion, additional power will be given to the above by uniting with them two or three grains of blue mass to be repeated every night for three or four nights, and subsequently every third or fourth night as long as may be deemed requisite or advisable for the individual case. In hemorrhage from the lungs, or uterus, small doses of ipecacuanha, combined with sugar of lead and opium are used with decided benefit.

In hemorrhage from the stomach, large doses of ipecacuanha have been strongly recommended, more particularly by Dr. Condie who has published some valuable practical remarks upon the subject.

In the early stages of the bowel affections of children, no less than in adults, an emetic of ipecacuanha, will often succeed in arresting the progress of the disease, and rarely fail to prove beneficial.

Combining from one fourth to half a grain of ipecac. with a minute portion of opium, and two or three grains of blue mass, the alterative properties of this last are materially enhanced, and will be found of great benefit in most of the mild cases of biliary and bowel derangements so prevalent in this region, at different seasons of the year.

With the exception of that sudden and often fatal disease, croup or hives, there are perhaps none of the pectoral diseases of children, in which the syrup of ipecacuanha may not be resorted to with advantage; but in croup, no little experience, and an almost uniform success in its treatment, authorize the confident belief, that we possess no one remedy or combination of remedies comparable or equal to the well known Coxe's hive syrup, provided it be properly prepared. Dr. Good has remarked, that the ipecacuans concur in operating very generally upon the skin, at the same time that they excite the stomach, increasing in a slight degree the discharge of mucus from the lungs, and adding a little to the peristaltic motion of the bowels, while the antimonials act more violently upon the stomach, bowels and skin, but less upon the mucous secernents.

To recur to the syrup of ipecacuanha, I may remark, that being

the United States Dispensatory attended with unnecessary trouble, and, without constant care, great probability of a want of uniformity in the preparation, I adopted, after many trials, the following formula, which can be depended upon at the bed side, and which has been found to keep well in this climate.

R.—Ipecacuan. Rad. Contus.	℥iv.
Aqua	O.ij.
Sp. Vin. Rect.	℥x.
Sacch. Alb.	lbs. iij.

Macerate the bruised ipecac in one pint of boiling water for 12 hours, then add the remainder of the water and alcohol, and continue the maceration for five or six days. Place the whole in a small displacement apparatus, returning the fluid that passes until it becomes perfectly clear, and then continue to pour a small quantity of water occasionally upon the surface, until two pints and ten ounces by measure shall have passed. Now add the sugar, and with a gentle heat, evaporate until the syrup shall be of a proper consistence, readily ascertained by occasionally taking out a small portion and allowing it to cool. When of a proper consistence, pass it through a small quantity of fine tow placed in the tube of a funnel to render the syrup clear and transparent. Three pints and ten ounces of syrup is the quantity obtained, and is in point of strength nearly double of that prepared by the usual formula, which I consider an additional recommendation.

N. O. Med. and Surg. Jour.

OBSTETRICS.

Turning by External Manipulation—The endeavor to rectify the faulty position of the fœtus in utero, by external manipulation, engaged much of the attention of the older practitioners, and especially of Wigand in 1807, but is scarcely alluded to in modern works on midwifery. Professor Martin, of Jena, has recently published an interesting essay upon the subject, based upon twenty-seven cases related by other authors, and seven which have occurred to himself. In these all the mothers did well, as also all the children but four, of which number, too, one was already dead, and another was delivered by perforation.

It is evident that an exact knowledge of the position of the child must be obtained before the attempt is made, and Professor Martin believes it is preferably acquired by external examination, although this implies a certain amount of dexterity. It is only prior to, or immediately after, the discharge of the waters, that the fœtus will be found possessed of sufficient mobility. As long as the os is undilated, and the pains irregular, the patient is kept on the side upon which the part desired to be forced into the pelvis is placed, so as to afford a moderate support. When, however, the os has become dilated, and the waters are expected to be discharged, she is laid (the bladder and rectum having been emptied) on her back the lower part of her body being somewhat raised. With one hand the operator presses the part of the fœtus which lies nearest the os, whether it be the head or breech, downwards,

while, with the other, he presses the rest of the body upwards. This simultaneous pressure is begun during the interval of a pain, and continued during its commencement, while, during the height of this, the uterus is firmly supported on every side. After a short pause the manipulation is again recommenced, and, if the operator's hands become tired, an assistant may, during the intervals of the pains, support the belly on each side, whereby the ovoid configuration of the uterus is better secured, and the child more easily brought into a proper position. If the pains are long absent, she may often be advantageously placed on her side, supporting the projecting part by the hand or a firm cushion. Especially is this position advantageous if manipulation has already somewhat improved the position, and it should, if possible, be continued after the head has entered the pelvis. For the purpose of retaining the head within the pelvis, when the improvement of the position is not durable, the rupture of the membranes, after the head or buttocks have been brought over the os, is a very efficacious procedure.

Among the rules to be observed, there is that of never employing a cold hand, for, not only is it disagreeable, but it may give rise to spasmodic action and a premature discharge of the waters. The pressure, too, should be moderate and continuous, and applied sometimes in a diffused manner with the flat hand, and at others by particular fingers, to various parts of the child. It must be always a double and simultaneous pressure, exerted equally towards the fundus and upon the part we are seeking to engage in the pelvis. A change in the position of the woman is often an important aid to the manipulation.

There are certain conditions requisite for rendering the operation of external turning an eligible one.—1. Absence of reasons for a rapid termination of labour, as hæmorrhage, presentation of funis. 2. Mobility of the child. Generally this ceases after a discharge of the waters. If, however, few pains have occurred, the presenting part has not become far forced down, or the uterus firmly contracted around it, a trial is justifiable. 3. Absence of great sensibility of the womb or abdomen. 4. Sufficient capacity of pelvis. A moderate degree of contraction is no contra-indication. 5. A normal activity of pains has been set down as a condition. It certainly is a very favorable one, but it is rare in faulty presentation, and not an essential one for external turning. Spasmodic or crampy pains which render the first two stages tedious, if due to cold, are best treated by mustard poultices and small doses of ipecacuanha. The very rectification of the position sometimes imparts a normal activity to the pains. External turning is rarely successful when the defective pains are dependent upon a rheumatico-catarrhal affection of the lower portion of the uterus giving rise to a softened state and premature rupture of the membranes. 6. The child being alive is a subordinate condition; for, although in the case of its death, internal turning may usually be advised, yet we very frequently cannot be certain of this.—*Dublin Med. Times, from Froriep's Notizien.*